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ART. XL.—*A Theory of the Formation of the great Features of the Earth's Surface;* by JOSEPH LECONTE, Prof. Geol. and Nat. Hist., University of California.

THE connection between the formation of continents and mountain chains on the one hand, and the phenomena of volcanoes and earthquakes on the other, is so evident and close, that this paper might, very appropriately, have been entitled "A general theory of igneous agencies." The general and first effect of igneous agency is, evidently, the formation of continents, sea bottoms and mountain chains. Volcanoes and earthquakes are secondary phenomena—they are but occasional accidents attending the slow march of these grander effects.

According to Humboldt, all the effects grouped under the general head of igneous agency are the result of "*the reaction of the interior on the crust of the earth.*" This formula, although far too vague and general to deserve the name of a theory, must, we believe, form the point of departure of every true theory. But in departing from this vague formula, only the most confused and contradictory notions seem to prevail amongst geologists. We have therefore thought that, on a subject of such vital importance, lying as it does at the very foundation of theoretic geology, any light, or even any more definite statement than now exists, might be considered timely. I have, for many years, thought much on the subject, and striven to emerge from the chaos which now exists into something like clearness of conception. I here present, with some hesitation, the results, hoping that they may serve, at least, as hints in the right direction.

The usual idea in regard to the general constitution of the earth is, that it is even now essentially a liquid, incandescent mass; the fiery liquid being separated from our feet only by a comparatively thin shell of solid matter. This solid shell, being lighter than the subjacent liquid, may be regarded as *floating upon it, and sustained by it.* Vertical upheaval or subsidence of the solid shell is supposed to produce continents and mountain chains on the one hand, or sea-bottoms on the other. Or else (as perhaps many would prefer), the contraction of the liquid interior being greater than that of the solid crust, causes the latter to wrinkle, i. e., to *bend into alternate convex and concave arches*, which form respectively the continents and sea-bottoms. The comparative *thinness* of the solid crust (only 25–50 miles according to these theorists) is supposed to make the process easy and the view plausible. On the contrary, it seems to us that this very supposed thinness of the solid crust renders the formation of continents and sea-bottoms *impossible*, except under very peculiar conditions. These conditions we now proceed to point out.

Continents and sea-bottoms.—If we regard the earth as consisting of a solid crust sustained as a floating body upon a liquid, the least reflection suffices to convince us that *the greater inequalities of the surface cannot be produced by alternate convex and concave bendings of the crust* like those shown in fig. 1, in which *a*

1.

is the continental and *b* the oceanic crust, and *ll* the sea level. No such arch as that producing a continent 3000 to 6000 miles across could sustain itself for a single moment: no; not if the crust were 100 or even several hundred miles thick. Still less would it be possible that the inverse arch, or concavity of ocean bottoms *b* (which in the case of the Pacific is 10,000 miles across), should sustain itself. The arch *a* would break *down* and the arch *b*, *up*, and the position of equilibrium *ll* would be assumed. So great is this force tending to the general form of equilibrium that, as shown by Thomson's reasoning referred to farther on, even if the earth as a whole were as rigid as a solid globe of glass, it could not resist it.* The irresistible and only conclusion, therefore, is that continents and sea-bottoms cannot be simple bendings or arches of a solid crust. If there be indeed a solid crust on a liquid interior, in order to sustain itself, the inequalities of the upper surface in contact with the air *must be repeated on the lower surface* in contact with the liquid

* Phil. Trans., May, 1862.

interior. The accompanying diagram (fig. 2) is an ideal representation of what *must be* the general character of the crust on this view. As before, *a* is the continental and *b* the oceanic crust. For simplicity's sake, in all these diagrams, the crust is represented as spread out on a plane. If we admit that the general constitution of the earth is that of a solid crust cover-

2.

ing a liquid interior, I cannot see how the above conclusion can be avoided.

Assuming then a general constitution of the solid crust like that represented in fig. 2, let us see how, on this theory, these inequalities might be formed. To form these, three conditions seem to be necessary. 1. The crust must be *lighter* than the interior liquid, i. e., it must be a floating body. We have assumed this all along, for there could not be a crust otherwise. 2. The material of the crust must *expand in solidifying*, i. e., in becoming crust. 3. Some portions of the crust must cool and thicken faster than others; these more rapidly thickening portions becoming the continents. Under these three conditions we may account for continents and sea-bottoms as follows.

Suppose a liquid earth consisting of heterogeneous materials, covered with a thin crust of solid matter, cooling and the crust thickening everywhere by additions to its lower surface. Evidently the more conductive portions would cool and thicken faster than the less conductive portions. Thus the inequalities would commence on the lower surface, as in fig. 3. But such

3.

a condition of things could not continue and increase: for if these areas of thicker and thinner crust be large, by the law of floatation the thicker parts *a a* must rise and the thinner portions *b* sink until equilibrium is established, and the condition of things represented by fig. 2 is assumed. A continuation of the same process, viz: the more rapid thickening of the continental portions *a a*, would cause these to rise higher and higher, and the ocean bottoms to sink deeper and deeper.

Thus, then, by this view, the formation of continents and sea-bottoms, and the increase in height and size of the former, and in depth of the latter, is due to *the unequal thickening of a floating crust by unequal cooling.*

Mountain chains.—If mountain chains were only *narrow wrinkles* on the earth's surface, we might suppose it possible

that they could sustain themselves as arches. But when we remember that they are in fact great plateaus or bulges a hundred or even a thousand miles wide, it seems impossible to avoid the conclusion that they too, if they rest on a liquid mass beneath, must be sustained by a *similar bulge on the lower surface* of the solid crust. The accompanying diagram (fig 4)

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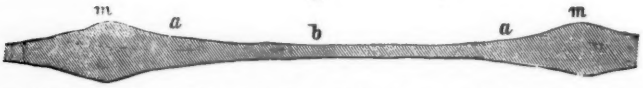

is an ideal section of the crust on this view, *a a* being, as before, the continents, *b* the sea bottom, and *m m* mountain chains. Of course in all the figures the thickness of the crust is enormously exaggerated.

The condition of the solid crust represented by figs. 2 and 4, viz., the existence of inequalities on the lower surface corresponding to the great inequalities of the upper surface, is, we believe, a demonstrably necessary consequence of the interior fluidity theory: there may be difference of opinion as to the *cause* of this condition, but there can be none as to the condition itself, if we admit the interior liquid.

We have given above what seemed on this theory the most probable *cause* of inequalities of crust thickness, viz: unequal surface cooling; but in order to state the interior-fluidity theory fairly, we will give another possible cause. In the gradual cooling of the earth, if the interior liquid should cool and contract faster than the solid shell (as it probably would), then the latter would be subjected to powerful horizontal thrust, to which it must eventually yield, and by which it would be thrown into wrinkles; *not, however, by bending up and down*, as is usually supposed (for this we have already shown is impossible), but *by crushing together and thickening in some places more than others.* By this view *continents and mountain chains and sea-bottoms are due to unequal thickening of the solid crust*, not by unequal cooling, as before supposed, but *by unequal crushing together by lateral pressure.* This mode of viewing has the great advantage of accounting for the foldings of strata so common in mountain chains. But on the other hand, it is difficult, nay impossible, to understand why, on this view, the thicker continental crust (made thicker by yielding), should continue to yield, rather than the thinner oceanic crust. In other words, it is impossible, on this view, to account for the undoubted general tendency of igneous agency to gradually and constantly *increase* the greater inequalities. Their tendency ought to be rather to destroy them as fast as formed, by the yielding and therefore thickening of the thinner portions, viz: the oceanic crust. This difficulty seems to be fatal.

The theory above presented, viz: the formation of continents and sea-bottoms by unequal thickening of a floating crust, by unequal cooling, may, with skillful modification and by adding hypothesis to hypothesis, be made to account for most of the phenomena of igneous agency in a somewhat satisfactory manner. In 1859, at the Springfield meeting of the American Scientific Association, I brought forward in some detail a theory of this kind. I soon became dissatisfied with it, however, as too hypothetical, and therefore never printed it. An abstract of it, however, I believe, appeared in the Canadian Naturalist. Subsequent reflection has induced me not only to reject it, but to doubt more and more the very basis upon which it rests, viz: the interior liquidity of the earth. To all theories based upon the assumption of a liquid interior and a floating crust, there are the following insuperable objections:

a. One of the necessary conditions of the formation of continents, &c., by unequal thickening due to unequal cooling, is a lightening of the material in the act of solidification. Now granite and other igneous rocks do not expand in the act of solidifying, but on the contrary, according to the experiments of Bischof, *notably contract.*

b. The solid shell of the earth, if there be any such, may be proved to be much thicker than is usually supposed by geologists of the interior-fluidity school. The principal argument for the liquidity of the earth beneath a comparatively thin shell is based upon the increasing temperature of the earth as we go downward into the interior. "The rate of 1° for every 50 feet of descent would give 3000°, the fusing point of iron, at a depth of 28 miles." At this temperature nearly all rocks would melt in our furnaces: therefore many hastily assume that the solid crust cannot be thicker than this. But all the calculations as to the thickness of the solid shell, based upon the rate of increasing interior temperature, neglect two important elements of the calculation, viz: the *increasing conductivity* of the earth as we descend, which would diminish the *rate* of increasing temperature, and the *increasing pressure*, which would elevate the fusing point. I have been accustomed for many years to illustrate the effect of these two distinct causes as follows:

Let S S (fig. 5) be the surface of the earth, and A B depth along any radius. Taking A B as an abciss, let the increasing heat be represented by ordinates. Now, if the density and conductivity of the earth were constant, then the heat would increase at uniform rate, and would therefore be correctly represented by the straight line C D. At the rate of 1° for every 50 feet we would reach the heat ordinate *t t* of 3000° and the melting point of rocks at the depth of 28 miles. But

since the density of the earth increases toward the center, it is almost certain that the conductivity increases also. The effect of this would be to diminish the rate of increasing temperature, which would therefore be correctly represented by a curved line C E, and not the straight line C D. Under these conditions the ordinate of 3000° on the curved line would not be reached at 28 miles, but at some point t' farther down, say 40 miles from surface. But having finally reached the temperature of 3000°, which we assume as the fusing point of rocks in our furnaces, we would, by no means, find the rocks in a state of fusion; for the enormous pressure of 40 miles of rock would undoubtedly very greatly elevate the fusing point. How much we know not; but let us suppose to 3500°. To find the ordinate of 3500° on the curve, we must go still deeper to some point t'', perhaps 60 or more miles from the surface. But here again we would fail to reach the lower limit of the solid crust, because the fusing point is again elevated above 3500° by the still greater pressure. And thus the fusing point flies before the increasing temperature; and *where* in this chase the former would overtake the latter, or *whether it would ever overtake it at all*, would depend on the rate of increase in the two cases. We have not at present the data to determine these.

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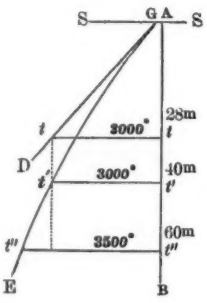

The conclusion from this reasoning is, that the crust is certainly much thicker than is usually supposed; so thick, indeed, that the focus of volcanoes and earthquakes must be *within its thickness;* and it even becomes doubtful whether there be any general fluid interior at all.

c. This doubt is entirely confirmed, according to the best physicists and mathematicians, by the effect of the sun and moon on the earth in producing *precession and nutation*, and in producing *tides*. In all these phenomena the earth, even under the most powerful distorting forces, *behaves like a perfectly rigid* solid, and not at all like a liquid or a partly liquid body. The reasoning of Hopkins, based upon the amount of precession and nutation, and his conclusion that the solid shell of the earth cannot be less than 1000 miles thick, is well known. By the majority of physicists and mathematicians it is regarded as sound. Nevertheless, doubts have been thrown upon it by men of high ability. But Thomson's argument, drawn from the behavior of the earth under the tide-generating influences of the sun and moon, is as yet untouched. It seems to be impregnable. The result of Thomson's reasoning is, that the

*earth as a whole is certainly more rigid than a globe of solid glass, and probably more rigid than a solid globe of steel.** We have already shown how fatally this extreme rigidity operates on any theory which attributes the inequalities of the earth's surface to bendings of a solid crust.

d. Very recently my attention has been directed by my brother, Prof. John LeConte, to another fact bearing strongly in the same direction, viz: the form of the equatorial section of the earth. If the earth were an exact spheroid of revolution, of course the mean surface of the equatorial section would be a perfect circle. Such, however, is not the case; the equatorial section is an ellipse whose major axis is about two miles longer than the minor axis. This, be it observed, is the form of the *equilibrium surface* or water-level; it therefore can be produced only by *unequal distribution of matter*, i. e., irregular density of the interior. Such irregular distribution of dense matter is easy enough to understand in a *solid earth;* for it would be the natural result of unequal cooling, and therefore unequal condensation, after the earth solidified. But it is difficult to understand how such irregular distribution of density could originate or how it could be maintained in a liquid. Humboldt† attributes it to *secular currents* in the interior liquid, by which denser matter is slowly transferred from one part to another. To say the least, this is a violent hypothesis, and I think would never have been thought of unless the interior liquidity of the earth was considered certain. Without insisting too far on this objection to the interior liquid, I think I am justified in saying that, on the assumption of a liquid earth, it is difficult to see why the interior density should not be perfectly symmetrically disposed, while on the assumption of a solid earth still cooling, the unsymmetrical disposition of density is almost a necessity.

There is, as far as we know, but one argument of a *general nature*, which has recently been brought forward in favor of the interior liquidity of the earth, viz: the three laws of earthquake occurrence rendered probable by Alexis Perry. The laws are as follows:

1. Earthquakes are more frequent when the moon is on the *meridian* than when she is on the *horizon:* 2. They are a little more frequent at the *syzygies* than at the *quadratures:* 3. They are a little more frequent when the moon is in *perigee* than when she is in *apogee.* If these laws are true, then it is evident that there is a slight tendency for earthquake occurrence to follow the law of *tides.* Many geologists seem to think that

* Phil. Trans., May, 1862. Thomson & Tait, Nat. Phil., p. 689.
† Cosmos, vol. iv, p. 19, Sabine's edition.

this, if established, is conclusive of the existence of an interior liquid in which tides are generated. On the contrary, it seems to us that, on the supposition of such an interior liquid, the effect of lunation on earthquakes and other igneous phenomena ought to be far more decided than it is. The very slight effect observed is, however, precisely what we would expect in a substantially solid earth. It is evident that in a solid earth, any force, whatever be its nature, tending to lift and break up the surface, would be *assisted by the moon on the meridian* and *repressed by the moon on the horizon;* and that these effects would be most marked at the syzygies and at perigee. Suppose then any force tending to elevate and break up any portion of the earth's surface, but resisted by the weight and the cohesion of the superincumbent strata; is it not evident that if these opposite forces were nearly balanced, the small lifting force produced by the moon in passing over might turn the scale in favor of the upward force, and thus determine the rupture of the strata, and therefore the occurrence of an earthquake?

The foregoing objections have induced many of the best geologists to believe that the earth is either solid or else that the solid crust is so thick that all igneous phenomena have their origin within the limits of that crust: and that therefore the earth may be regarded as *substantially solid.* The liquid ejections of volcanoes, according to this view, must be regarded as having their origin in local accumulations of liquid matter which have no connection with a general fluid interior. I confess, my own sympathies are entirely with this modern school of geology. It seems to me that while much of the prestige of great names, such as Humboldt, Von Buch, Elie de Beaumont, &c., are on one side, most of the substantial argument is on the other. I feel convinced that the whole theory of igneous agencies—which is little less than *the whole foundation of theoretic geology—must be reconstructed on the basis of a solid earth.* What follows is an attempt in this direction.

1. *Continents and Sea-bottoms.*

Suppose a solid earth of oblate spheroid form and even surface, covered with a universal ocean and cooling by radiation. If the material were homogeneous and therefore of equal conductivity along every radius, then the cooling and the consequent contraction along each radius would be equal, and, so far as this cause is concerned, the earth, though becoming smaller, would maintain its symmetry of form and its universal ocean. But such homogeneousness could not be expected, nor does it exist. In a heterogeneous earth thus cooling, *areas of*

greater conductivity would cool more rapidly and therefore *contract more rapidly* in a *radial direction.* These more conductive areas, with shorter radii A C, A C (fig. 6), would form the sea-bottoms, while the less conductive and therefore the less radially contracted portions B B would become land surfaces. The accumulation of water on the shorter or more contracted radii would not check the process; for although water is a bad conductor, yet it conveys heat from bottom to surface by *convection* with great rapidity, and therefore the process of cooling through a stratum of water would go on far more rapidly than through a stratum of any rocky material. The same process therefore continuing, would tend constantly to increase the inequalities thus commenced. In other words, the sea-bottoms would sink and the continents increase in size and height. With many oscillations difficult or impossible at present to account for such has, on the whole, been the progress of change during the whole geological history of the earth.

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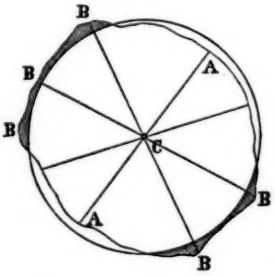


The law of fluid equilibrium requires that in the pre-existing fluid condition, and therefore also when the earth first became solid, the quantity of matter along each radius was nearly or quite equal. This equality would not be affected by the subsequent unequal contraction. It seems probable, therefore, that the same equality still exists, and that, therefore, the *matter along the shorter oceanic radii is denser* than along the longer continental radii. If, farther, we suppose great mountain plateau-masses also to be formed by unequal radial contraction, then the plateau radii would contain the lightest matter of all. According to Archdeacon Pratt,* the form of the Indian arc indicates unmistakably that continental matter and especially Himalayan matter is less dense than sub-oceanic matter. His view of the origin of the great inequalities of the earth seems to be something like what I have presented, since he states his belief that the absolute quantity of matter along different radii is the same. I have stated this as a *possible* mode of formation of great plateau-masses. As a general fact, however, I account for mountain chains in an entirely different manner, which I now proceed to explain.

* Phil. Mag., vol. xli, p. 307, 1871.

2. *Mountain Chains.*

A cooling earth may be regarded as composed of concentric isothermal shells, each cooling by conduction, and in the case of liquid also by convection to the next outer and the outermost by radiation into space. Furthermore under these conditions, at first and for a long time, the outermost shell would cool fastest; but there would eventually come a time when the surface having become substantially cool, and moreover receiving heat from external sources (sun and space), as well as internal, its temperature would become *nearly fixed*, while the interior would *still* continue to *cool* by conduction. This has probably been the case during the whole *recorded* history of the earth. Now, therefore, the interior portions cooling, and therefore *contracting*, *more rapidly* than the exterior, the latter would be subjected to powerful *horizontal pressure*, which continuing to increase with the progressive interior contraction, the *exterior* portion *must* eventually *yield* somewhere. *Mountain chains are the lines along which the yielding of the surface to horizontal thrust has taken place.* But observe: according to my view, this yielding is not by up-bending into an arch leaving a hollow space beneath, nor such an arch filled and supported by an interior liquid; but *a mashing or crushing together horizontally, like dough or plastic clay, with foldings of the strata, and an upswelling and thickening of the whole squeezed mass.*

No other view but this will, I think, satisfactorily explain the complex foldings so universal in great mountain chains. We will not stop, however, to discuss this point now, but hasten to give what seems to us nothing less than demonstrative proof of this crushing together and up-swelling of mountain chains. This proof is found in the phenomenon of *slaty cleavage.*

It is now established beyond the possibility of a doubt, by the beautiful observations and experiments of Sharpe, Sorby, Haughton and Tyndall, that *slaty cleavage is produced by* powerful *pressure* perpendicular to the planes of cleavage, by which the whole rock-mass has *been mashed together* and shortened in that direction, and *correspondingly extended* in the direction of the planes of cleavage. As the planes of cleavage are usually highly inclined or even vertical, it is evident that the rock-mass has been crushed together *horizontally* and *swelled up vertically.* As a necessary consequence of this crushing together, we find associated with cleavage the most complex foldings not only of the strata, but of the layers and even of the finest lines of lamination. This *plication is always associated with cleavage*, and, *vice versa*, cleavage, when the rock material is suitable for devel-

oping this structure, is always associated with plication; and both are associated with mountain chains.

A mashing together horizontally and an extension vertically, or an up-swelling of the mass, is therefore proved in slaty cleavage, and thus also in mountain chains where slaty cleavage occurs. It only remains to show that the *amount* of mashing in one direction and extension in the other, absolutely proved in these cases, *is fully adequate to account for the upheaval of the greatest mountain chains.*

By the observations of Sorby and Haughton, taking an ideal *cube* of the original *unsqueezed mass*, the ratio of the greatest diameter (in the plane of cleavage), to the least diameter (perpendicular to cleavage), becomes, after squeezing, sometimes 2 : 1, sometimes 4 : 1, 6 : 1, 9 : 1, and even sometimes 11 : 1. According to Sorby, the average is 5 or 6: 1. Now it is evident that of the three rectangular diameters of such a cube, one vertical and two horizontal, one of them, the *horizontal* in the direction of pressure, would be *shortened*, another, the vertical, would be *elongated*, while the third, the horizontal at right angles to pressure, would be *unchanged*, because in the rock-mass yielding could not take place in that direction. It follows then that the *change* of the original diameters in either direction, by compression or elongation, would be the *square roots* of the ratios mentioned above. Thus if a cube of 3 inches diameter be crushed together horizontally, and allowed to extend only vertically, until its previously equal diameters become as 9: 1, it is evident that the vertical diameter has been increased and the horizontal diameter diminished 3 times. Taking 6: 1 as the average ratio in cleaved slates of diameters originally equal: we may assert that *in cleaved rocks the whole mass has swelled up* 2½ (2·45) *times its original thickness.* Suppose then a mass of sediments 10,000 feet thick subjected to horizontal pressure and crushing sufficient to develop well-marked cleavage structure; *a breadth of* 2½ *miles would be crushed into* 1 *mile, and* 10,000 *feet thickness would be swelled to* 25,000 *feet, making an actual elevation of the surface of* 15,000 *feet.* Now we actually have strata not only 10,000 but 20,000, and even 40,000 feet thick.

I think, therefore, I am justified in asserting that the *phenomena of plication and of slaty cleavage demonstrate a crushing together horizontally, and an up-swelling of the whole mass of sediments; and that slaty cleavage demonstrates in addition that the up-swelling produced by this cause alone is sufficient to account for the elevation of the greatest mountain chains.*

[To be concluded.]

ART. XLI.—*Letter to the Superintendent of the U. S. Coast Survey, containing a Catalogue of bright lines in the Spectrum of the Solar Atmosphere, observed at Sherman, Wyoming Territory, U. S. A., during July and August*, 1872; by Prof. C. A. YOUNG, of Dartmouth College.

Prof. B. PEIRCE, Supt. U. S. C. S., &c., &c.

Dear Sir—Without waiting to complete my entire report of the spectroscopic work at Sherman, I send for immediate publication, should you think proper, a list of the bright lines observed in the spectrum of the chromosphere during the past summer.

The great altitude of the station (nearly 8,300 feet), and the consequent atmospheric conditions, were attended with even greater advantages for my special work than had been really expected, although I was never quite able to realize my *hope* of seeing all the Fraunhofer lines reversed, unless once or twice for a moment, during some unusual disturbances of the solar surface.

Everything I saw, however, confirmed my belief that the origin of the dark lines is at the base of the chromosphere, and that the ability to see them all reversed at any moment depends merely upon instrumental power and atmospheric conditions.

In this view, a catalogue of the bright lines actually observed is of course less important than it would be otherwise; still it is not without interest and scientific value, since the lines seen are naturally those which are really most conspicuous in the chromosphere spectrum, and this conspicuousness stands in important, but by no means obvious or even entirely simple, relations to the intensity of the corresponding dark lines, when such exist. There can be no doubt that a careful study of these bright lines and their behavior would yield much valuable information as to the constitution and habitudes of the solar atmosphere.

In the catalogue, the first column contains simply a reference number: a ‡ refers to a note at the end of the catalogue.

The numbers in the second column refer to my "Preliminary Catalogue," containing 103 lines, which was published a year ago in the "American Journal of Science." In this column a † indicates that some other observer has anticipated me in the determination and publication of the line. As I have depended for my information almost solely upon the Comptes Rendus and the Proceedings of the Royal Society (which give the observations of Lockyer, Janssen, Rayet and Secchi), it is quite possible that some other lines ought to be marked in the same manner.

The third column, headed K, gives the position of the lines on Kirchoff's scale, the numbers above G being derived from Thalen's continuation of Kirchoff's maps. In this column an asterisk denotes that the map shows no corresponding dark line, a ? that the exact position, not the existence, of the line is for some reason slightly uncertain.

The fourth column, headed A, gives the wave-length of the line in ten millionths of a millimeter, according to Angström's atlas.

The numbers in this and the preceding column were taken, not from the maps themselves, which present slight inaccuracies on account of the shrinking and swelling of the paper during the operation of printing, but from the numerical catalogues of Kirchoff and Angström which accompany their respective atlases. In the preliminary catalogue the numbers were derived from the maps; hence some slight discrepancies in the tenths of division.

The fifth column, marked F, contains a rough estimate of the percentage of frequency with which the lines were seen during the six weeks of observation; and the sixth column, B, a similar estimate of their maximum brightness compared with that of the hydrogen line C.

The variations of brilliance, however, when the chromosphere was much disturbed, were so considerable and so sudden that no very great weight can be assigned to the numbers given. Nor is it to be inferred that lines which have in the table the same index of brightness were always equally bright. On certain occasions one set of lines would be particularly conspicuous; on others, another.

With two or three exceptions, indicated in the notes, no lines have been catalogued which were not seen on at least two different days. In the few cases, where lines observed only on one occasion have been admitted to the list, the observations were at the time carefully verified by my assistant, Prof. Emerson, so as to place their correctness beyond a doubt. Many other lines were "glimpsed" at one time and another, but not seen steadily enough or long enough to admit of satisfactory determination.

The last column of the catalogue contains the symbols of the chemical elements corresponding to the respective lines. The materials at my disposal are the maps of Kirchoff and Angström, Thalen's map of the portion of the solar spectrum above G, and "Watts' Index of Spectra." Since the positions of the lines in the latter work are given only to the nearest unit of "Angström's scale," I have marked the coincidences indicated by it with a (w), considering them less certain than those shown by the maps.

In addition to the elements before demonstrated to exist in the chromosphere, the following seem to be pretty positively indicated—sulphur, cerium and strontium; and the following

with a somewhat less degree of probability, zinc, erbium and yttrium, lanthanum and didymium. There are some coincidences also with the spectra of oxygen, nitrogen and bromine, but not enough considering the total number of lines in the spectra of these elements, or of a character, to warrant any conclusion. One line points to the presence of iridium or ruthenium, and only three are known in the whole spectrum of these metals.

No one, of course, can fail to be struck with the number of cases in which lines have associated with them the symbols of two or more elements. The coincidences are too many and too close to be all the result of accident, as for instance in the case of iron and calcium, or iron and titanium.

Two explanations suggest themselves. The first, which seems rather the most probable, is that the metals operated upon by the observer who mapped their spectra, were not absolutely pure—either the iron contained traces of calcium and titanium, or *vice versa*. If this supposition is excluded, then we seem to be driven to the conclusion that there is some such similarity between the molecules of the different metals as renders them susceptible of certain synchronous periods of vibrations—a resemblance, as regards the manner in which the molecules are built up out of the constituent atoms, sufficient to establish between them an important physical (and probably chemical) relationship.

I have prefixed to the catalogue a table showing the number of lines of each substance, or combination of substances, observed in the chromosphere spectrum; omitting, however, oxygen, nitrogen and bromine, since with one exception (line 230), neither of them ever stands alone, or accounts for any lines not otherwise explained.

The instruments and methods of observation were the same employed in the construction of the Preliminary Catalogue. Telescope, $9\frac{4}{10}$ inches aperture—spectroscope automatic, with dispersive force of 12 prisms.

The approximate geographical position of Sherman is Lon. $1^h\ 53{\cdot}2^m$ west of Washington; Lat. 41° 07′; Altitude above sea-level 8,280 feet; mean height of barometer about 22·1 inches.

Table showing the number of coincidences between the bright lines observed in the spectrum of the chromosphere, and those of the spectra of the chemical elements.

				Unknown.	52	Total,
Fe, Ti, $S_{(w)}$	1			Fe,	64	110
" Ba, $S_{(w)}$	1	Ti, $S_{(w)}$	3	Ti,	23	43
" $S_{(w)}$ $Zn_{(w)}$	1	" Ca,	2	Ca,	10	29
" Co, Ce,	1	" Mn,	1	Ba,	8	13
" Ni, $E_{(w)}$	1	" Ce,	1	$S_{(w)}$	7	14
Ca, Cr, Ce,	1	" Sr,	1	Mn,	6	12
" Li, Zn,	1	" Zn,	1	Ce,	5	11
Ti, Ba, $S_{(w)}$	1	Ca, Cd,	1	H,	4	4
Ba, La, $E_{(w)}$	1	" Ce,	1			

TABLE—continued.

Fe,	Ca,	10	Ca, Co,	1	Na,	4	6
"	Ti,	9	" Cr,	1	Cr,	4	10
"	Mn,	4	" Sr,	1	Mg,	3	4
"	Cr,	3			Sr,	3	6
"	Ni,	3	$S_{(w)}$ $E_{(w)}$	1	Zn,	3	9
"	Ba,	2			$E_{(w)}$	2	9
"	Zn,	2	Mn, Zn,	1	Ni,	2	6
"	$E_{(w)}$	2			Co,	1	5
			Cr, $E_{(w)}$	1	Cu,	1	2
"	Ce,	1			La,	1	3
"	Co,	1	Ce, Co,	1	Ru, Ir,	1	1
"	Mg,	1			Cd,		1
"	Na,	1	Na, Cu,	1	Li,		1
"	$S_{(w)}$	1					
"	La,	1	lines mark'd with a *	14			

The numbers in the last column denote the whole number of times that the symbol of each element appears in the catalogue, either singly or combined with others.

Catalogue of bright lines in the Spectrum of the Chromosphere. 1872.

No.	P. C.	K.	A.	F.	B.	E.	No.	P. C.	K.	A.	F.	B.	E.
1‡	1†	534·0*	7055·?	100	12		38	11†	D_1 1002·8	5895·0	50	30	Na,
2	2†	654·3	6676·9	25	50	Fe, $Ba_{(w)}$	39	12†	D 1006·8	5889·0	50	30	Na,
3	3†	C.694·1	6561·8	100	100	H,	40‡	†	1011·2	5883·0	2	1	Fe,
4		711·4	6515·5	15	4		41‡	13†	D_2 1016·5*	5874·9	100	90	
5	4	718·7	6496·0	18	5	Ba,	42		1031·8	5852·7	8	2	Ba,
6		731·7	6461·7	5	2	Ca.	43		1135·1	5708·3	1	1	Fe,
7	5	734·0	6453·8	10	6		44		1151·1	5687·2	2	1	Na,
8		740·9	6438·1	5	2	Ca, Cd,	45		1154·2	5683·5	5	3	
9‡	6	744·3	6429·9	20	4		46		1155·8	5681·5	2	1	Na, Fe, $N_{(w)}$
10		750·1	6415·6	5	2		47		1165·7	5667·8	2	2	$S_{(w)}$
11		756.9	6399·0	5	2	Fe,	48		1167·0	5666·0	1	1	
12		759·3	6392·6	5	1	Fe,	49		1170·6	5661·5	15	2	Fe, Ti, $E_{(w)}$
13		767·?*	6373·?	5	2		50		1175·0	5656·7	8	3	$S_{(w)}$ $N_{(w)}$
14	7	768·?*	6371·?	5	3		51		1176·6	5654·4	2	1	Fe,
15		778·3	6346·1	10	4	Ruth, Ir,	52		1187·1	5640·2	1	1	$S_{(w)}$
16‡	8	823·5	6245·4	8	5	Fe,	53		1189·3	5637·3	1	1	
17‡	9	827·6	6237·3	8	2		54		1200·6	5623·2	2	1	Fe,
18		830·2	6231·5	5	1	Fe,	55		1207·3	5614·5	2	1	Fe,
19		836·5	6218·3	3	1	Ti,	56		1229·6	5587·6	2	2	Ca,
20		839·2	6214·1	3	1	Ti,	57		1231·3	5585·5	2	1	Fe,
21		845·7	6199·6	2	2	Fe,	58	14†	1274·2	5534·1	50	12	Ba, Fe, Sr,
22		849·7	6190·5	10	2	Fe,	59	15	1281·3	5525·9	40	5	Fe,
23		859·7	6168·3	3	1	Ca,	60		1287·5	5518·7	15	2	Ba,
24		863·9	6161·2	8	3	Ca,	61		1298·9	5505·8	2	1	Fe,
25		870·9	6148·1	3	2	Fe, $E_{(w)}$	62		1303·5	5500·5	2	1	Fe, La,
26		871·4	6146·8	3	2		63		1306·7	5496·6	2	1	Fe, $E_{(w)}$
27	10	874·3	6140·6	25	10	Ba,	64		1320·6	5480·2	2	1	Ti, Sr,
28		876·5	6136·1	2	1		65		1324·8	5475·9	1	1	Ni,
29		877·0	6135·6	2	1	Fe,	66		1328·7	5472·3	3	1	
30		884·9	6121·2	5	3	Ca, Co,	67		1337·0	5462·3	1	1	Fe, $N_{(w)}$
31		890·2	6109·9	2	1	Ba,	68	16	1343·5	5454·7	10	4	Fe,
32		894·9	6101·7	3	2	Ca, Li, $Zn_{(w)}$	69	17	1351·1	5445·9	10	4	Fe, Ti, $Br_{(w)}$
33		903·1	6083·1	3	2	Ti,	70		1360·9	5435·4	5	2	Zn, $Br_{(w)}$
34		912·1	6064·5	5	2	Fe, Ti,	71		1362·9	5433·0	2	2	Fe,
35		933·8	6018·0	2	1	Ba,	72‡	18	1364·3	5431·8	8	5	
36		949·4	5990·0	10	4		73	19	1367·0	5428·8	8	3	Fe, Ti,
37		992·0	5913·2	2	1	Fe,	74	20	1372·1	5424·5	25	6	Ba, Ti, $S_{(w)}$

TABLE—continued.

No.	P C	K.	A.	F.	B.	E.	No.	P C	K.	A.	F.	B.	E.
75	21	1377·4	5417·9	5	2	Ti, Mn,	133		16'7·4	5195·0	1	1	Mn,
76		1380·5	5414·4	2	2	Fe,	134		1618·9	5194·1	2	2	Fe,
77	22	1382·5	5412·4	4	2	$Mn_{(w)}$	135		1627·2	5188·2	10	5	Fe, Ca,
78		1384·7	5410·0	2	1	Fe, Ni,	136		1628·2	5187·3	1	1	Ti,
79		1385·7	5409·0	2	2	Cr,	137		1631·5	5185·1	5	2	Fe, Ti,
80		1389·4	5404·8	2	1	Fe,	138	49†	$b_1$1634·1	5183·0	50	30	Mg,
81	23	1390·9	5403·1	5	3	Fe, Ti,	139	50†	$b_2$1648·8	5172·0	50	35	Mg,
82		1394·2	5399·6	2	1	Mn,	140	51†	$b_3$1653·7	5168·3	40	30	Fe, Ni, $Br_{(w)}$
83	24	1397·5	5396·1	4	2	Fe, Ti,	141	52†	$b_4$1655·6	5166·7	30	20	Fe, Mg,
84		1401·6	5392·2	2	1	Fe, Ce,	142		1666·?	5160·?	1	1	
85		1412·5	5380·2	3	2	Ti,	143		1671·5	5154·8	3	1	Na,
86	25†	1421·5	5370·5	10	3	Fe,	144	53	1673·7	5152·5	3	1	Na, Cu,?
87		1423·0	5369·0	1	1	Fe,	145	54	1677·9	5150·1	2	2	Fe, $Br_{(w)}$
88		1425·4	5366·5	1	1	Fe,	146		1689·5	5142·2	1	2	$S_{(w)}$
89		1428·2	5364·0	1	1	Fe,	147		1701·8	5133·0	1	1	Fe,
90	26†	1430·1	5361·9	20	10	Fe,	148		1704·7	5130·8	1	1	Fe,
91		1438·9	5352·4	4	2	Fe, Co, Ce,	149		1707·9	5128·6	1	1	Ti,
92		1446·7	5345·0	1	1		150		1710·7	5126·7	1	1	Fe, Ti,
93		1450·8	5340·2	1	2	Fe, Mn, $O_{(w)}$	151		1712·2	5125·5	1	2	
94	27	1454·7	5335·9	5	2	Ti, $Zn_{(w)}$	152‡		1713·4	5124·4	1	1	Fe,
95		1461·5	5329·1	6	4		153		1715·2	5123·2	1	1	Fe,
96	28	1462·8	5327·1	5	2	Fe,	154		1717·9	5121·0	1	1	Fe,
97	29	1463·3	5327·6	5	2	Fe,	155		1719·4	5119·9	1	1	Ti,
98		1464·8	5325·1	6	2		156‡		1727·3	5114·9	1	1	Ni,
99		1471·9*	5318·0	1	1		157		1734·6	5108·8	2	2	$Ti_{(w)}$
100‡	31†	1473·9	5315·9	90	50	Fe,? $O_{(w)}$?	158		1737·7	5107·0	1	1	Fe,
101		1476·8	5313·1	3	1		159		1750·4	5098·1	1	1	Fe,
102		1497·3	5292·0	1	1	Cu, $Br_{(w)}$	160		1752·8	5096·5	1	1	Fe, $S_{(w)}$
103	32	1505·3	5283·4	20	10	$Ti_{(w)}$	161		1765·0*	5087·0	2	1	$E_{(w)}$
104	33†	1515·5	5275·0	30	15		162		1771·5	5083·5	1	1	$Zn_{(w)}$
105	34	$E_1$1522·7	5269·5	15	4	Fe, Ca,	163	55	1778·5	5077·9	1	2	Fe,
106	35	$E_2$1523·7	5268·5	12	3	Fe,	164		1823·6	5047·8	2	2	Fe,? $Zn_{(w)}$
107	36†	1527·7	5265·8	10	4	Fe, Co,	165		1833·4	5041·2	2	2	Fe, Ca,
108		1530·2	5263·3	1	1	Ca, $Br_{(w)}$	166		1834·3	5040·1	2	2	Fe,
109		1538·5*	5256·2	2	1	Sr,	167		1848·9	5030·1	4	3	$S_{(w)}$
110		1541·9	5254·1	1	2	Fe, Mn,	168		1856·9	5023·5	3	1	$S_{(w)}$
111		1547·7	5249·7	3	1	Fe,$Z_{(w)}Br_{(w)}$	169	56†	1867·1	5017·6	30	15	Fe. Ni,
112		1551·6	5246·3	3	1	Fe,	170	57†	1870·6	5015·0	30	10	$Ti_{(w)}$
113	37	1561·0	5239·0	4	2	Fe,	171		1905·1	4993·3	2	1	Fe, $N_{(w)}$
114	38	1564·2	5236·3	4	2		172‡		c 1961·0	4956·7	1	2	Fe,
115	39†	1567·5	5233·6	10	8	Mn, $Zn_{(w)}$	173	58†	1989·5	4933·4	30	8	Ba,
116	40	1569·6	5232·1	1	3	Fe,	174	59†	2001·6	4923·1	40	12	Fe,$S_{(w)}Zn_{(w)}$
117		1575·4	5227·5	1	1	Sr,?	175	60†	2003·2	4921·3	30	8	$S_{(w)}$
118	41	1577·4	5226·2	10	3	Fe,	176	61	2007·2	4918·2	20	3	Fe,
119		1578·1	5225·5	2	3	Sr, $Br_{(w)}$	177		2016·0	4911·2	3	2	$Zn_{(w)}$
120	42	1580·1	5224·3	2	2	Ti,	178	62†	2031·1	4899·3	30	6	Ba, La, $E_{(w)}$
121		1589·1	5216·5	2	1	Fe,	179	63†	2052·5?*	4882·9	10	4	Ce,
122		1590·7	5215·5	3	2	Fe,	180		2067·8	4869·4	5	1	
123		1592·3	5214·4	2	1	Fe,	181	64†	F 2080·0	4860·6	100	80	H,
124		1597·9*	5210·5	1	1		182		2087·6	4854·7	5	2	Fe, Ni, $E_{(w)}$
125		1598·9	5209·5	1	2	Ti,	183		2094·0	4848·1	3	2	Ca, $O_{(w)}$
126	43	1601·5	5207·6	10	6	Fe, Cr,	184		2116·7*	4826·5	1	1	
127	44	1604·4	5205·2	10	6	Cr, $E_{(w)}$	185		2121·2	4822·8	10	2	Mn,
128	45	1606·4	5203·7	10	6	Cr, Fe,	186		2142·4	4804·4	3	1	Ti, $S_{(w)}$ $O_{(w)}$
129	46	1609·2	5201·5	5	3	Fe,	187		2171·5	4778·7	3	2	Co, $N_{(w)}$
130	47	1611·3	5199·7	4	2	$S_{(w)}$ $E_{(w)}$	188		2229·1	4730·8	1	1	Fe,
131		1613·9	5197·9	1	1	Fe,	189‡		2251·3*	4712·5	2	2	Ce, $O_{(w)}$
132	48†	1615·6	5197·0	15	10		190		2309·5	4666·3	3	1	Fe, Ti,

TABLE—continued.

No.	P C	K.	A.	F.	B.	E.	No.	P. C.	K.	A.	F.	B.	E.
191		2314·3	4663·3	2	1		233		2680·0	4407·7	1	1	Fe, Ca,
192		2323·0	4656·0	2	1	Ti,	234		2686·8	4404·2	1	1	Fe,
193	65	2358·4	4629·0	15	8	Ti, N (w)	235		2696·0	4398·5	1	1	Ti, Ce, O (w)
194		2359·5*	4628·2	2	1	Ce,	236		2698·2	4396·5	1	2	
195		2369·7	4620·3	1	1		237	87	2702·5	4394·6	15	3	
196		2410·2	4589·4	1	1		238		2715·2	4388·5	1	1	Fe?
197		2412·8	4587·5	2	2		239	88	2718·5	4384·7	8	2	Ca, Ce,
198	66	2419·3	4583·2	15	6		240		2720·2	4383·5	1	1	
199		2429·5	4576·0	4	2		241	89	2721·6	4382·8	1	1	Fe, Cr,
200	67	2435·5	4571·4	10	4	Ti,	242		2725·8	4380·4	1	1	
201	68	2443·9	4564·8	10	3		243		2728·0	4379·1	1	1	Ca,
202	69	2446·6	4563·2	10	5	Ti,	244	90	2733·7	4375·5	5	3	Fe,
203		2452·1	4559·5	8	2		245	91	2736·9	4374·2	8	3	E (w)
204		2454·1	4558·1	8	1		246		2762·0	4359·1	1	1	Cr,
205	70	2457·9	4555·3	10	5	Fe, Ti,	247	92	2775·7	4351·8	3	1	Cr,
206	71	2461·2	4553·4	10	5	Ba,	248	93†	2795·7	4340·1	100	65	H,
207		2463·4	4551·8	1	1	Ti, S (w)	249		2798·0	4338·2	10	2	Cr,
208	72	2467·6	4548·9	10	8	Ti,	250		2805·4	4335·1	2	1	La,
209		2480·8	4539·2	2	1	Ce,	251		2823·4	4324·0	1	2	
210	73	5486·6	4535·5	2	2	Ti, Ca,	252		2830·7	4320·1	1	1	Ti, O (w)
211	74	2489·4	4533·2	5	5	Fe,	253		2843·0	4313·5	1	1	Ti,
212		2490·5	4532·1	3	2	Ti, Ca,	254	94	G. 2854·2	4307·2	3	2	Ca, Fe.
213	76	2502·2	4524·4	3	2	Ba, Fe,	255	95	2867·7	4302·1	3	2	Ca, Fe,
214	77	2505·6	4522·0	3	3	Ti, S (w)	256	96	2874·2	4298·0	1	1	Ca, Fe,
215		2517·0	4514·0	2	1		257	97	2894·5	4289·4	1	1	Cr, Ca, Ce (w)
216		2518·4	4513·0	1	1		258	98	2928·5	4274·6	2	1	Cr, Ca,
217		2527·0	4506·0	2	1		259	99	2961·2	4260·0	2	1	Fe,
218	78	2537·1	4500·3	15	6	Ti,	260	100	2996·2	4245·2	30	3	Fe,
219	79	2552·4	4490·9	20	8	Mn,	261		3018·0	4235·5	30	5	Fe,
220	80	2555·0	4489·4	15	3	Fe, Mn,	262		3022·8	4233·0	15	5	Fe, Ca,
221	81	2566·3	4480·9	5	2	Mg,	263	101	3040·0	4226·3	3	3	Ca, Sr,
222‡	82†	f 2581·2	4471·2	100	25	Ce,	264	102	3061·8	4215·3	40	7	Ca, Sr,
223	83	2585·4	4468·5	20	5	Ti, O (w)	265		3155·5	4178·8	1	1	
224		2620·8	4446·3	1	1	Ti,	266		3187·0	4166·7	1	1	Ca,
225	84	2625·2	4443·0	10	2	Ti,	267	103†	h. 3363·5	4101·2	100	50	H,
226		2633·0	4436·7	1	1	Mn?	268		3431·0	4077·0	25	2	Ca,
227		2639·6	4433·5	1	1		269		3526·0	4045·0	3	2	Fe,
228		2651·5*	4426·0	2	3		270		3703·3	3990·?	2	1	
229		2653·2	4425·0	2	2	Ca,	271		3769·5	3970·?	2	1	Fe,
230		2664·9	4418·0	2	1	O (w)	272‡		H, 3778·5	3967·9	75	3	Fe, Ca,
231		2665·9	4417·5	3	1	Ti,	273‡		H, 3882·5	3932·8	50	1	Fe, Ca,
232	85	2670·0	4414·7	1	1	Fe, Mn, O (w)							

NOTES.

1. The position assigned to this line, first observed by Respighi (a fact of which I was ignorant when the Preliminary Catalogue was published), rests upon two series of micrometric measurements, referring it to four neighboring dark lines—the probable error is about $\frac{1}{10}$th of a division of Kirchoff's scale.

9. No. 6 in P C. Position there given, 743?

16 and 17. Nos. 8 and 9 of P C. Position given as 816·8 and 827·6, by a mistake in identifying lines upon the map.

40. I have never myself seen this line reversed. Prof. Emerson, however, saw it several times. It was first reported by Rev. S. J. Perry, in *Nature*, vol. iii, p. 67.

41. The position of this line has been independently determined by three series of micrometric comparisons with neighboring lines. My result agrees exactly with that of Huggins.

72. Erroneously given in P C, as 1363·1, which line does not reverse, or at least was never seen reversed at Sherman.

100. The principal line in the spectrum of the corona. The corresponding line in the spectrum of iron is feeble, and on several occasions when the neighboring lines of iron (1463, &c.), have been greatly disturbed, this has wholly failed to sympathise. Hence I have marked the Fe with a ?. Watts indicates a strong line of oxygen at 5315 A.

152 and 156. Observed only on one day, but verified by Prof. Emerson.

172. Called 'little C' by Mr. Stoney.

179. Given by Lockyer as K 2054. Its position is a little uncertain; it seems to coincide with neither of the dark lines at 2051 and 2054, but lies between them, a little nearer to 2051.

189. Rather a band than a line.

222. The position of this line, which, however, like 189, is rather a band, was determined by two series of careful micrometrical measurements. It was discovered by Rayel in January, 1869; since called 'f' by Lorenzoni.

272 and 273. These lines were both reversed (by a narrow bright stripe running down the center of the broad hazy band) as constantly, whenever the seeing was good, as *h* or C itself. The observation was difficult, however, and required the most scrupulous exclusion of foreign light, and a careful adjustment of the slit in the plane of the solar image formed by these particular rays.

They were also found to be regularly reversed upon the body of the sun itself, in the *penumbra and immediate neighborhood of every important spot.*

ART. XLII.—*On the Quartzite, Limestone and associated rocks of the vicinity of Great Barrington, Berkshire Co., Mass.;* by JAMES D. DANA. With a map, on Plate IV.

WHATEVER the appearances at Canaan,* the quartzite in Great Barrington (or Barrington, as it is called in the guide-books), a dozen miles north of Canaan, alternates with mica schist, and *both of these rocks for the most part overlie the "Stockbridge" limestone.*† The quartzite is, therefore, as well as the schist, to a great extent a newer rock; and as both occur in some places interstratified with upper beds of the limestone, all are parts of one consecutive series of strata.

If then the western range of limestone in Berkshire is, like that of West Rutland, Chazy‡ in age, the range to the eastward

* See, on the "Canaan Quartzite," this Journal. III, iii, 179. I shall at another time reconsider the conclusion in that paper with regard to the relations of the quartzite to the other rocks; it was based on general considerations and not on observed sections.

† This relation of the gneiss and quartzite was observed by Prof. Hitchcock, and is stated in his Massachusetts Geological Report (pp. 588–590, 1841), where he says that the quartzite might be considered a member of the gneiss and mica schist formation. Its relation to the associated limestone he speaks of, on p. 590, as "little more than a juxtaposition." But on p. 573 he had stated correctly that inasmuch as the limestone was interstratified with the same schist, it therefore must be also with the quartzite. The areas colored as *quartzite*, on both the Massachusetts and Vermont geological maps, are largely, and some *wholly*, mica schist, gneiss, or hydro-mica slate. This is the fact with the Great Barrington region as well as others.

‡ This volume, page 133, in a note by Mr. E. Billings.

is probably Trenton; and, further, the quartzite and the associated mica schist (and gneiss), since they directly overlie the limestone, and besides are sometimes interstratified with it, are, with little doubt, either of the Trenton, or of the Cincinnati (Hudson River) group. Prof. Hitchcock makes the statement that the limestone of the western range is essentially pure carbonate of lime, or contains very little magnesia if any, while the more eastern, both in Massachusetts and Vermont, is mostly dolomite.*

On the western border of Berkshire (or of Massachusetts) stretch along the Taconic mountains, which consist in these latitudes, on the eastern side, chiefly of a fine-grained mica slate, sometimes chloritic, and occasionally staurolitic as well as garnetiferous. The rocks dip eastward, with local exceptions, and underlie conformably the western range of Berkshire limestone. The limestone adjoining these Taconic slates on the east being Chazy, the slates themselves are almost certainly of the period next preceding, and therefore Quebec schists, as made by Logan.

Hence, in this part of the Green Mountain region, the rocks are probably a continuous series of Lower Silurian strata, commencing with the Quebec group, and ending seemingly with the upper group of this era—the Cincinnati or Hudson River group. These views accord in the main with those of the geological map of Sir Wm. E. Logan, that is, in making the beds Lower Silurian, and the Taconic slates to represent the Quebec group; but differ in referring the other rocks (on the basis of facts since ascertained) to the Trenton and perhaps also the Hudson River period. They sustain as regards the limestones the view put forward as early as 1841, by Professors W. B. and H. D. Rogers.† They are entirely at variance with the views of Prof. Emmons, who made the limestone, schists and quartzite, all older than the Potsdam. They are inconsistent with the view that in Canada and Western New England an epoch of disturbance intervened between the Trenton era and that preceding it; and support the old conclu-

* How far this difference is connected with difference of geological age has not been ascertained. Hitchcock states that successive layers in the same region are sometimes unlike in this respect, and T. S. Hunt found the same to be true in the Quebec group of Canada. Chemical analyses to settle the question would have, therefore, to be very numerous. In the east-and-west region including North Adams and Williamstown, the marbles of Williamstown, the *western*, are magnesian, according to Hitchcock's Massachusetts Report, while those of North Adams, or the *eastern*, contain little or no magnesia.

† Amer. Phil. Soc., Jan. 1, 1841. This Journ., xlvii, 151, 1844. The Messrs. Rogers made the limestone Trenton, or Chazy and Trenton, but the quartzite they pronounced Potsdam, and the Taconic slates, with those west of them to the Hudson River, Hudson River slates.

sion of Logan, that the great disturbances affecting these Lower Silurian rocks occurred at the close of the Lower Silurian.

I propose to discuss the facts as to the special age of these rocks on another occasion.

I have said that the conformable superposition of the limestone, schists, and quartzite is unquestionable—contrary to the idea with which I entered upon the study of the Green Mountain rocks. The evidence consists in the direct testimony of sections showing unmistakable superposition.

In my discussion of the subject, I first briefly explain the *topography* of the region, in order that the positions and geographical relations of the several localities described may be appreciated. The accompanying map (Plate IV) in its outlines is a reduction of the Berkshire County map of 1858; the topography I have added from such observations as I have been able to make without the use of instruments. I next describe the *kinds of rocks* in order that their variety and their transitions may be understood, and also the bearing of the facts on the value of lithological characters as a criterion in geological chronology. Finally, I shall present the facts connected with the *stratification* of the rocks, as obtained from sections in different parts of the region.

1. *Topography.*

In the vicinity of Great Barrington (see the map) there are three principal north-and-south valleys over a region about ten miles wide, each containing strata of crystalline limestone.

(1.) The *western*, that of Egremont (and western part of Great Barrington), is backed, as just stated, by the Taconic mountains.

(2.) The *central* is that of the Housatonic river, on which are situated the villages of Great Barrington; Van Deusenville, two miles north of Great Barrington; Housatonic, two miles farther north; and Stockbridge, three miles northeast of Housatonic, or six from Great Barrington.

(3.) The *eastern*, the valley of Muddy Brook and the Konkapot, lies along the eastern border of the town of Barrington, and has Beartown Mountain on the east.

The two ranges of ridges between these three valleys consist mainly of the metamorphic schists and quartzite which overlie, or are interstratified with, the limestone.

The range bounding the Housatonic valley on the *west* is, by estimate, 100 to 250 feet in height above the river. North of Van Deusenville it consists of two overlapping parts, L and W, and between them flows Williams river.

The range on the *east* of this valley varies from 100 to at least 500 feet in elevation above the Housatonic. To the northward it bends more to the westward toward the Housa-

tonic river and the village of that name, narrowing the valley there to less than a fourth of a mile; in this part the eastern range is a broad elevated region, 300 to 450 feet in height above the river by estimate, and is called Monument Mountain. Southward also the ridge increases in height, and three miles (by the road) east of Great Barrington ($2\frac{1}{2}$ miles in a direct line, at T and farther south) becomes "Three Mile Hill," or ridge. The high land of "Three Mile Hill" extends westward to "East Mountain," just east of Great Barrington, reducing abruptly the width of the Housatonic valley in this part to half a mile.

Beartown Mountain is probably full 600 feet above the plain at its western base. It is a ridge chiefly of gneiss, and of the same age with the other ridges just described.

The Egremont plain or region, on passing north of the latitude of Van Deusenville, is divided into three north-and-south valleys. The Tom Ball ridge bounds the town of Alford on the east, and, extending northward into West Stockbridge, divides the southern half of the latter town into an eastern and western section. "Tom Ball," the highest summit of the range, lies directly west of Williamsville, and is by estimate 750 feet in elevation above the plain either side. Its rocks are mica or hydro-mica slate and chloritic mica slate, like those of the Taconic range. Again, the town of Alford is divided longitudinally from north to south by a low ridge of the same slates, 50 to 250 or 300 feet in elevation.

I may also state, as it will be necessary to make use of the fact in the course of this memoir, that a range, similar in rocks and altitude to the Tom Ball ridge, lies farther east along the *eastern* boundary of the town of West Stockbridge, and extends northward between Richmond and Lenox to the southern border of Pittsfield. "Lenox Mountain" is its highest part, and may well give a name to the ridge. All the ridges here mentioned have an approximate parallelism to the Taconic range farther west.

The limestone region extends to the eastward of Beartown Mountain. I propose to continue my investigations of the Taconic and later rocks in that direction another season. How far they spread east, whether anywhere to the Connecticut river valley or not, is yet among the unknown things in American geology.

2. *Kinds of Rocks.*

The following rocks occur in the limestone region within 20 miles north or south of Great Barrington. The beds of schist and slate either directly underlie, or are interstratified with, or overlie the limestone; the quartzite rocks all overlie the lower limestone stratum, but are sometimes interstratified with others above.

A. *Limestone.*—The limestone of the region is, in general, a fine-grained crystalline rock, white, bluish-white, or clouded. It graduates in some places into a slate-blue variety, which is but slightly crystalline. At one place along the railroad, a mile west of Stockbridge, I looked, with some hope of success, for fossils; and I think I found one, although the form somewhat like the head of a Cystidean with its stem, is not distinct enough to entitle it to credit. The same bed at times varies in the course of a mile or less, from a blue semi-crystalline rock into a whitish crystalline. Pyrites and minute brown tourmalines are present in some impure beds. Tremolite is a very common mineral in the Canaan limestone, where a massive form is the so-called *Canaanite;* it also occurs in the next town, Sheffield, but is sparingly met with in the limestone farther north. Brown mica in scales is also common in the less pure layers of the Canaan limestone.

B. *Metamorphic schistose rocks and slates.*—The following are among the occurring kinds of schistose and slaty rocks.

(1) *Mica schist*, abounding in mica, the scales largish, their colors white and black intermingled, the latter predominating.

(2) *Arenaceous mica schist*, with minute scales of mica.

(3) *Mica slate*, using this term in a sense distinct from that of mica schist, for a shining slaty rock, smooth in surface, intermediate between mica schist and clay slate, and also between mica schist and hydro-mica slate. It is a prominent constituent of the Taconic Mountains, and also of Tom Ball and other summits in the limestone area. This rock is in some places *staurolitic* and *garnetiferous.*

(4) *Hydro-mica slate*, like the mica slate, but feeling greasy, and owing this peculiarity to the presence of a hydrous mica instead of the ordinary anhydrous. It is not always easy to draw the line between this slate and mica slate, except by chemical analysis. The slate of Tom Ball and that of the Taconic Mountains have in part the external characters of this rock. [Near Rutland, Vt., a variety, very decidedly soapy to the touch, occurs associated with, and graduates into, quartzite, and all *overlie* apparently the limestone of the region.*]

* I repeat here that the hydro-mica slate was formerly called *talcose slate*, and by Emmons *magnesia slate;* later by T. S. Hunt, in the Canada Geological Reports, *nacreous shale;* and by Hitchcock in the Vermont Geological Report of 1861, *talcoid schist.* It was shown by Hunt (Can. G. Rep. 1854, 1863) and G. F. Barker (Vt. G. Rep., p. 708) to contain no magnesia.

The following are analyses; 1 by Hunt; 2 to 4 by Barker:

	Si	Al	Fe	Mg	Ca	Na	K	vol.
1. Ste Marie, Canada, *red*,	66·70	16·20	6·90	2·65	0·67	[3·78]		3·10=100
2. Irasburgh, Vt., *gray*,	78·70	12·80	Fe *tr.*	*tr.*	1·23	5·57	0·89	0·60= 99 79
3. Roxbury, Vt., *gnh-gy.*,	69 90	20·00		1·80	1·51	2·33	1·45	2·40= 99·39
4. Pownal, Vt., *bh-gray*,	42·90	42·20		1·98	0·78	1·33	5·24	5·60=100·03

These analyses indicate that it is often impossible to distinguish the hydro-mica slates; for No. 2, although described in the Vermont Report as "the most unc-

(5) *Chloritic* varieties of the mica or hydro-mica slate occur in Tom Ball and the Taconic Mountains, in some of which there is as much chlorite as mica. They sometimes contain crystals of magnetite. [At the Rutland locality, referred to above, there is a true dark green *chlorite schist*, containing magnetite associated with the hydro-mica slate and quartzite.]

(6) *Fine-grained gneiss*, grayish to bluish gray in color; the feldspar and quartz in small grains and distinguishable from one another with difficulty. The fusibility of the rock shows that it contains feldspar. The rock is sometimes thick-bedded and of uniform color. A bluish gneiss of this kind is quarried for architectural purposes on Monument Mountain. A very common variety is strongly and closely banded with black and white through the alternation of very micaceous and quartzo-feldspathic layers. It affords thick slabs, and is quarried west of Great Barrington and elsewhere. A very *contorted gneiss* of this kind occurs, which, although containing much mica, has lost, through the contorting, all its schistose structure, and almost all traces of its bedding.

(7) *Granitoid gneiss*, a light colored, even-grained granite-like rock breaking into great blocks, the mica in small scales, mostly black. (Overlies mica schist and quartzite).

(8) *Whitish gneiss*, containing very little mica in distinct interrupted streaks and spots.

(9) *Hornblendic schist*, interstratified with the gneiss, of a blackish colo..

(10) *Garnet rock*, interstratified with gneiss and gneissic quartzite, a dark gray rock, exceedingly tough, made up largely of pale reddish massive garnet, with quartz, and perhaps some feldspar (though not distinguishable), and few minute oblong whitish crystals of some hornblendic mineral. Contains minute particles of pyrites.

(11) *Tremolite rock.* The Canaanite of Canaan, a coarsely granular rock, white to grayish-white in color, often mixed with quartz and with limestone, and often containing scales of brown mica. Weathered specimens sometimes show the ends of crystals of tremolite.

(12) *Clay-slate* occurs alternating with quartzite and limestone in Williamstown, Massachusetts, and at Rutland and elsewhere in Vermont. That of the Taconic range west of Great Barrington lies mostly to the west of the mica or hydro-mica slate of that range.]

tuous of all the specimens analyzed," evidently contains, chiefly if not solely, an anhydrous mica, and this a *soda* mica, if there is any mica present. The analysis gives too little alumina in proportion to the alkali for any known species. No 4 appears to be almost solely the hydrous mica damourite; just such a rock has been recently described from Salm-Château, and called damouritic schist. Another analysis by Barker, not here cited, is of a chloritic variety.

C. *Quartzite beds.*—The rocks of the quartzite beds differ much in character. The principal kinds are the following.—

(1.) An intensely hard, gray or whitish quartzite, jointed profoundly and in more than one direction, but without distinct traces of bedding. Some beds are a conglomerate of the same hardness, made up of pebbles or stones from the size of a pea to that of cobble stones. Minute particles of pyrites are sparingly disseminated through a large part of this and other varieties of the quartzite.

(2.) A rock equally massive in fracture and almost as firm, but showing the bedding more or less distinctly. (It is often used for the hearths of furnaces.) Cleavable particles of a glassy feldspar are sometimes distributed through it. When thus laminated it often contains, especially over the surfaces of the laminæ, scales of a white mica or hydro-mica, and sometimes minute brown or blackish tourmalines.

A rock of this kind often weathers rapidly on exposure, so as to become very friable, or even fall to sand. (It is used for making glass in the region.)

(3.) *Soft sand-beds*, in thin layers, that change deeply to a dirty sand.

(4.) *Calcareous quartzite*, which graduates on one side into limestone and the other into quartzite. Some hard laminated quartzites are very porous as if they had once contained calcareous material.

(5.) *Gneissic quartzite* and *quartz-conglomerate.* A variety consisting partly of quartz pebbles half an inch to an inch in diameter containing large masses of orthoclase and much mica, and really a variety of gneiss.

(6.) *Feldspathic quartzite*, a quartzite, often very hard, containing much orthoclase through its mass. The orthoclase decomposes easily and becomes removed, leaving the rock cavernous, and thus, as Hitchcock long since explained, is produced the *buhrstone* of Berkshire. Besides the orthoclase, a glassy cleavable less alterable feldspar may often be distinguished in the so-called buhrstone, and sometimes in the walls of the cavities that had been made by the decomposition and removal of the orthoclase; it is probably either albite or oligoclase.*

The transitions between the different kinds of rock in the quartzite formation are often very abrupt. Only a few rods sometimes separate the regularly-bedded fragile quartzite from the hard bedless granular quartz. The soft sand-rock often has within it intensely hard masses made up of the sands of

*I am indebted for specimens of this and the preceding variety of the quartzite to Dr. Stephen Reed of Pittsfield. They are from the valleys of Roaring Brook and Mill River, east of New Lenox.

many layers solidified together; the bedding stops short off in a mass of structureless quartzite, several yards, it may be, in breadth and height, which lies isolated, so that it might be taken for an intruded boulder were it not for the traces of bedding on some parts of its exterior.

When the rock of a quartzite stratum is the harder quartzite, it is sure to stand out over the region in bold crests, or make the precipitous brow of a mountain ridge. When it is the softer variety, the ridges may have gentle slopes, and lie deeply covered with earth, showing nowhere an outcrop of the beds. But ridges of this latter kind often have vast numbers of quartzite boulders over the surface, which one ignorant of the facts would suppose were all transported boulders, while they are partly at least the hard knots of the rotted sand rock.

These diversities in the quartzite serve to explain much that is mysterious in its apparent distribution. It does not answer the purposes of strict science to set down the plains along the valleys as all limestone areas; for the soil of these plains may rest on soft beds either of the quartzite formation, or of the mica schist; and we cannot infer from an outcrop of hard quartzite only a few yards in breadth that the concealed stratum below to which it belongs has no greater breadth.

Again, *following the direction of the bedding*, there are sometimes changes from quartzite to mica schist or gneiss. This is proved by the fact that the strata of the same north-and-south range, or in the direction of the strike, are mainly quartzite in one place, and in another, two or three miles off, are mainly schist. It is also suggested by finding a limestone stratum overlaid by schist with quartzite above the schist at one point, and in another not far distant by quartzite directly, the schist being wanting. This kind of evidence (to be given in detail in the continuation of this memoir) is not as positive as that from direct observation along exposed strata. Direct proof of the plainest kind exists, as I have seen, in the quartzite ridge northeast of Rutland, Vt. (colored as quartzite in the Vermont Geological map, Rep. of 1861); the schist (here mainly hydro-mica schist) is the prevailing rock of the ridge, and in some places it passes in the course of a hundred yards into true quartzite. The transition is so obvious and complete that no occasion exists for doubting with regard to analogous transitions in Berkshire.

These facts at the first thought seem strange. But we take little note among unaltered stratified rocks of the change along the bedding from a sandstone of purely siliceous sand to an impure sandstone; or, off an existing seashore, from the sands of a sand-flat to the mud of the shallow bottom but a few rods

distant; and the difference we find in the Green Mountain rocks is only this small and often unimportant distinction made intensely apparent by metamorphism.

If then a bed of rock may be quartzite in one part and mica schist or gneiss in another, and if these rocks alternate with one another in the way mentioned, *there is not strictly any quartzite formation in the Green Mountains*; for the formation is made up of various rocks, and quartzite is not always the predominant one.

The kinds of rocks in the region under discussion have been here separately described with some detail because the fact is not generally appreciated that gneiss, granitoid gneiss, coarse and fine mica schist, hydro-mica slate, compact garnet rock, hornblende slate, chloritic rocks, as well as quartzite, soft and hard, may belong to the Stockbridge limestone formation, and even overlie it. Many of the rocks are precisely such as belong to the so-called "Green Mountain Series," which series has been pronounced on lithological evidence to be *pre-Silurian* and *Huronian*.

I have collected specimens of chloritic mica slate from the summits of Mt. Washington in the Taconic range; of Tom Ball; of the Graylock ridge, near South Adams, Mass.; of Mt. Mansfield, in the region of the Green Mountain series of rocks in Vermont; and from the ridge two miles west of this city (New Haven, Ct.), which are so closely alike as to appear as if broken from the same block or slab. Are these all of the same age? all Huronian or pre-Silurian? or all Trenton or Hudson River rocks? I have found staurolites (which Hunt makes as good as a fossil for distinguishing one series of pre-Silurian rocks) in the Taconic slates of Salisbury, Conn., underlying directly the Canaan limestone: Is the rock *therefore* of the White Mountain series and pre-Silurian? I have seen a slate abounding in staurolites alternating with hornblende rock, gneiss and quartzite, in Vernon, in southeastern Vermont, but a few miles north of the Bernardston region of either *Lower Helderberg* or *Devonian* quartzite, slate and limestone (crinoids an inch in diameter of stem occurring in the beds), and, as the Vermont Report states, the quartzites of these adjoining towns are probably the same rock: Are *these* beds of the White Mountain series and pre-Silurian?

We learn from the facts how much virtue there is in lithology for determining the equivalency of metamorphic rocks. It may afford a quick answer to hard questions, but its answer is worth very little unless otherwise abundantly fortified.

[To be continued.]

ART. XLIII.—*On the nature and duration of the discharge of a Leyden Jar connected with an induction coil;* by OGDEN N. ROOD, Prof. of Physics in Columbia College. Part III.

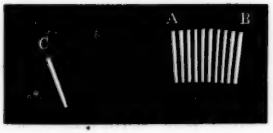
(Concluded.)

Form and duration of the discharge of the small jar with brass balls as electrodes.

The form of the discharge under these circumstances was more complex; the succession of acts being generally as follows: first, an instantaneous spark, which was followed by a pale violet discharge, lasting a small fraction of a second; afterward came a series of instantaneous sparks whose number diminished as the striking distance became greater. Fig. 5 shows about the appearance with the mirror, fig. 6, with the disc, the latter giving more reliable information regarding the actual number of discharges. The length of the simple induction spark in these experiments was 57·6 millimeters.

5.

6.



Striking distance 1 *millimeter.*—The total duration was somewhat variable; the average of the experiments were,

Average with the mirror,	·0247 sec.
" " " disc,	0276 "
Final average,	·0261 "

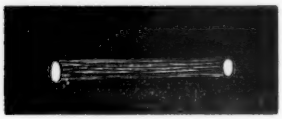
The portion A B, figs. 5 and 6, consisted of from ten to twenty instantaneous sparks, its duration varying with the number of included discharges. Disc observations gave for

Maximum duration of AB,	·0119 sec.
Medium " " "	·0090 "
Minimum " " "	·0053 "

Taking then ·0090 as the medium duration, and fifteen as the medium number of discharges, we have for the average interval separating the individual sparks ·0006 sec. This will give for the medium duration of the violet portion CA ·017 sec.

Other forms.—A quite different form was sometimes present, like that in fig. 7, and consisting of a faint violet streak terminated at each end by an instantaneous spark, the duration being, ·026 sec. to ·030 sec.

7.



These, from their rarity, were a little difficult to study, and I am not sure that in all cases the act was *terminated* by a spark. The violet streak was of course invisible in disc experiments.

Striking distance 2 *millimeters.*—The form was the same as fig. 5, but the dimensions were somewhat reduced.

Total duration with mirror,.....·01830 sec.
" " " disc, ...·01588 "

Average, ...·01709 "
Average duration of portion AB, ... ·005 "

The other form (fig. 7) also sometimes occurred with a duration of ·027 sec.

Striking distance 3 *millimeters.*—The form was still the same; the portion AB consisting sometimes of only three or four sparks, but often of eight or ten.

Total duration with mirror,.....·0124
" " " disc, ...·0125

The form of fig. 7 sometimes occurred, apparently with a duration about twice as great as that just given.

Striking distance 4 *millimeters.*—The form was sometimes the same as those just given, but more often consisted solely of a compact series of from four to six instantaneous sparks.

Duration with four sparks,.....·0043 sec.
" " six " .....·0082 " } disc.

Striking distance 5 *millimeters.*—Merely two, three or four isolated sparks.

Average total duration,.....·0035 sec. (disc).

Striking distance 6 *millimeters.*—Same form as the last,
Average total duration,.....·0025 sec. (disc).

Striking distance 7 *millimeters.*—Two, three, rarely four sparks.
Average total duration,.....·0018 sec. (disc).

Striking distance 8·75 *millimeters.*—Only two sparks, one quite faint. Interval between them ·0009 sec. (disc).

Striking distance 10 *millimeters.*—One spark; seldom two. 10·75 was the maximum striking distance; at it, and half a millimeter under it, only a single spark was produced.

Form and duration of the discharge of the small jar with platinum points as electrodes.

The length of the simple induction spark was 58 millimeters.

Striking distance 1 *millimeter.*—The form varied considerably, three or four different kinds being mingled. The simplest form consisted of ten or twelve instantaneous sparks following each other at a pretty regular interval, which increased somewhat toward the close of the act. This kind of discharge produced a short hissing sound like that obtained by thrusting a red hot wire in cold water, and its presence could be detected by the ear alone. The total duration of this form was subject to considerable variation; below I give

The greatest,	·0099 sec.
" least,	·0037 "
" medium,	·0068 "

The last quantity was not obtained as the mean of the other two, but is the average of eleven distinct experiments with the disc and mirror. These I give as a sample of the variation in the results sometimes obtained in these experiments, the discordance being mainly due, not to the methods, but to the phenomenon itself.

sec.	
·0047	mirror and compass.
·0099	" " "
·0064	" " "
·0047	" " mercury tube.
·0064	" " " "
·0074	disc.
·0092	"
·0079	"
·0037	"
·0085	"
·0063	"
Average ·0068	

If we take the average number of sparks as ten, we shall have for the average interval separating them ·0007 sec., that is, these sparks were generated at the rate of 1428 per second. I suppose that a combination of ten or fifteen of them would be sufficient to render the ear sensible of the *tone* produced; but owing to the irregularity of their position, instead of furnishing an approximation to a musical tone, the sound resembled rather that of the combination of the consonants, ST. By swiftly drawing a card or piece of thin sheet brass over the edge of an irregularly notched plate of brass, this sound could be imitated.

Other forms.—Quite often the discharge was like that given in fig. 7, with a total duration of ·017 sec. More rarely the form in fig. 5 was produced, but I could obtain no good measurement of it. In *one* case its average duration was doubtfully estimated to be ·005 sec.

Striking distance 2 *millimeters.*—Form was generally like fig. 4, merely ten or twelve instantaneous sparks; total duration ·0075 sec. Sometimes forms like fig. 7 were seen.

Striking distance 3 *millimeters.*—Like the last, with perhaps twenty sparks; total duration ·0088 sec. Forms like fig. 7 were sometimes seen.

Striking distance 4 *millimeters.*—Fifteen or twenty sparks; total duration ·0081 sec.

Striking distance 5 *millimeters.*—Form same as the last; number smaller; total duration ·0073 sec.

Striking distance 7 *millimeters*—Same form; total duration ·0067 sec.

Striking distance 9 *millimeters.*—Form the same; total duration ·0060 sec.

Striking distance 10 *millimeters.*—Same form; total duration .0046 sec.

Striking distance 12 *millimeters.*—Same form still; only four or five sparks; total duration ·0041 sec.

Striking distance 15 *millimeters.*—Like the last, consisting of two or three sparks, total duration ·0020 sec.

The maximum striking distance was 24·5 millimeters, when the discharge consisted of only a simple instantaneous spark, and this was true, down to distances as small as 21 millimeters.

It will be noticed that the total duration of the discharge in these experiments was not, as formerly, a maximum at the smallest striking distance, but became so at 3 millimeters, which I am disposed to account for by the circumstance, that at the smaller striking distance, many of the heavier discharges assumed the form of figure 7. Some general correspondence between the results with brass balls and platinum points will also be noticed.

These experiments show that a single excitation of the coil supplies sufficient electricity to repeatedly charge jars of moderate or small dimensions, this happening not only on account of the amount of electricity generated, but also because the act of its production is spread out over a certain interval of time. In this connection, then, it becomes of interest to examine and measure this interval in the absence of the jar, and to ascertain its relation to the total duration of the multiple discharges which have just been described. It has long been known that the simple induction discharge consists of an instantaneous spark which is followed by a violet light, the latter lasting during a rather large fraction of a second. This is called the *aureol*, and has been studied by several physicists.

As their results, nevertheless, would have no particular application to the matter in hand, I made a new set of experiments with the same apparatus, using also the same electrodes and battery.

Duration of the aureol with brass balls as electrodes.

Striking distance.	Duration.
1 millimeter.	·026 sec.
2 "	·015 "
3 "	·012 "
4 "	·009 "
5 "	·006 "

At a striking distance of ten millimeters the aureol was not visible. The light of the aureol corresponding to one of the electrodes was violet, that due to the other had a hue approaching red. With small striking distances the two streaks were in contact, but separated as the distance between the electrodes was increased.

Duration of the aureol with platinum points as electrodes, the length of the simple induction spark being 48·7 millimeters.

Striking distance.	Duration.
1 millimeter.	·022 sec.
2 "	·020 "
3 "	·018 "
10 "	·011 "

With ten millimeters only one of the streaks was visible; as before, they were red and violet.

In both of these experiments the duration of the aureol increases as the striking distance diminishes, and it is evident that with distances under a millimeter still greater durations would probably be obtained. Now while the durations in these experiments for a striking distance of one millimeter do not represent the actual time during which the induction current flowed into the jar, they do prove that it occupied at least as great an interval as ·026 sec., which is the maximum duration of the multiple discharges described in this paper.

The general result obtained in these experiments may then be summed up as follows: if a Leyden jar, of a ected size, with a fixed striking distance, be connected with an induction coil, the act of exciting the coil will charge and discharge it at *once;* as the size of the jar is diminished, it will be charged and discharged oftener, and the aureol will begin to mingle itself with the spark discharges, and as the size still further diminishes, will finally replace all of them except the first; for it is evident that we may regard two brass balls, with an intervening layer of air a millimeter thick, as a minute Leyden jar, representing the last of the series.

The combination of the aureol or violet discharge with the multiple sparks, is, I think, to be explained thus: the first spark heats and rarefies the air between the electrodes; if then the electric current is furnished with sufficient rapidity by the coil, the tension of the electricity in the jar may rise sufficiently for a discharge through the rarified air before it can cool down, and thus produce the violet light. This state of things would continue till the electricity began to be furnished more slowly by the coil, when it would result that, the air between the electrodes having time to cool down would no longer permit the electricity to pass through it in an unbroken stream, but would compel it to discharge itself in sparks. According to this idea the successive sparks ought to be separated by a gradually increasing interval, and this indeed appears to be the case, the rise of the separating interval being particularly strongly marked toward the close of the total act. Brass balls favor this mixed form of discharge, perhaps, by confining the air to a certain extent, and thus preventing it from cooling down. Platinum points can have no such influence, and with them this phenomenon is rare. With the larger jar it never occurred, as the air was always able to cool down before the electric tension had sufficient time to rise in its larger surface, so as to warrant a discharge. With the small jar and longer striking distances, the violet light was not produced for an analogous reason, and also perhaps because the air was less confined.

In using the multiple sparks for investigations where an instantaneous illumination is required, the presence of the aureol-like discharges cannot occasion trouble, owing to their slight luminosity and large duration, the latter being enormous when compared with that of the spark. In the experiments described in the second part of this paper, this violet light was never visible although it must often have been present, and I was enabled to make the determinations here given only by a reduction of the distance between the mirror and the ground glass, and the use of a lens of vastly larger angular aperture. The large number of sparks (15 or 20), given by the small jar with a suitable striking distance, is admirably adapted for certain purposes of investigation, and enables the experimenter to dispense with arrangements for controling the moment at which the discharge take splace. If single, non-multiple sparks are desired, the indications I have given will readily guide to their production, it being always preferable to test with a revolving disc making fifteen or twenty revolutions per second, and provided with a small open sector of $\frac{1}{4}°$.

I will also call the attention to the circumstance that where Leyden jars in connection with induction coils have been used

in experiments on binocular vision, etc., the results are not to be relied on as having been obtained with an instantaneous illumination, as is abundantly shown by the large total durations so often met with in this investigation.

Finally, by the use of large rotating discs, these multiple discharges can readily be exhibited to moderate sized audiences.

New York, June 29th, 1872.

ART. XLIV.—*On the Allegheny System of Electric Time Signals;* by Prof. S. P. LANGLEY.

THE necessity of a uniform standard of time for the railways of the United States is one which is growing into importance with the increasing extent of our railway system; and we are, ere long, in this country, to be called on to settle for ourselves a practical problem which has already been solved in England, and which is beginning to make its demand for solution upon the managers of our railroads.

Since past experience shows that their probable adoption of a new and common standard will introduce it to public notice and discussion, and then to adoption by cities and individuals, it is desirable that this should not be done without the direction which intelligent scientific coöperation will give to a movement originated by the demands of intercontinental traffic. As few are aware how generally this coöperation has already been invoked, nor to what extent the public is indebted to observatories for increased security of transit, it has seemed that an account of what has been done in this direction in any one of them would be of interest.

The earliest introduction of the system of electric automatic transmission of time-signals, on an extended scale, appears to be due to the observatory of Greenwich.

The Astronomer Royal, with Mr. C. V. Walker, commenced their use in 1852, carrying for that purpose special wires on the poles of the South Eastern Railway from Greenwich to London Bridge. The subsequent extension of the use of Greenwich time under this system has been almost universal throughout the United Kingdom, the observatories of Glasgow and Liverpool, under the direction respectively of Professor Grant and Mr. Hartnup, as well as that of Edinburgh, having taken part in bringing it to its present condition of utility. For an instructive and very full description of the methods employed at Greenwich, reference may be made to an article in the Horological Journal for April, 1865, by W. Ellis, Esq., F.R.A.S., to whom, as to all the gentlemen named, the writer

has been indebted for much kindly-given information on the results of their long experience.

Although the introduction of the plan in this country has been comparatively recent, the number of American observatories which thus distribute time, is so considerable that the most partial account of their methods, and the extent of their work, would exceed the limits of such an article as the present. In this, the only arrangements described are those in use at the Allegheny Observatory, with which the writer has become familiar from the responsibility of their initiation and superintendence. It is proper to add, that were he writing a history of the progress of electric time signals in the United States, other observatories which have before employed not dissimilar means, would receive earlier mention, and that his own part in introducing these signals at the Allegheny Observatory has been less the contribution of any novel device, than an adaptation of what seemed the best features of plans in use abroad, their arrangement in a form adapted to the needs of American railways, and the supervision of their application to the wants of cities and individuals.

In doing this, a great number of ingenious devices have been examined, and if the system to be described appears to be one of the simplest, it has yet been reached only after much care in setting aside all which would not bear the test of practical trial.

The subject was first specially considered at the Allegheny Observatory some three years since, and a plan was arranged for the managers of the Pennsylvania Central Railroad in 1869. Previously to this, however, at the request of some jewellers of Pittsburg, the time had been transmitted to their stores, at a distance of some miles from the observatory. The system now described has been in use for nearly three years, in furnishing the Pennsylvania Central Railroad with its official standard of time; and by it the time is now sent daily to Philadelphia on the east, as far as Lake Erie on the north, and to Chicago on the west, regulating the clocks on a number of minor roads over whose wires it goes, as well as on those of the principal southern lines connecting the Atlantic with the Mississippi. Thus passing, as it does, over several thousand miles daily, it is believed to be at present one of the most extended systems of time distribution in the world.

The observatory is on the summit of the ascent on the northern side of the valley of the Ohio, about two miles in a direct line from the offices of the Western Union Telegraph Company in Pittsburg, and rather more from those of the Pennsylvania Central, and Pittsburg, Fort Wayne, and Chicago roads. It is connected with these points by three independent lines of telegraph.

One of these runs to the Western Union offices, and thence to the stores of a considerable number of jewelers in Pittsburgh. This is called the "Jewelers' line." The second, connecting the observatory through the offices mentioned with Eastern Pennsylvania and New Jersey Railways, and also with Chicago, is known as the "Railroad line." The third, consisting of a double wire or "loop," communicating with the city, is employed occasionally for the observatory's own messages, and when (as, for instance, in longitude determinations) it is wished to send sidereal time, without interrupting the regular transmission of signals from the mean-time clock. In the transit room, in the western wing of the observatory, are kept the sidereal clock by Frodsham, of London, and the principal mean-time clock by Howard, of Boston.

On the escape-wheel arbor of this, the standard mean-time clock, and turning with it, once a minute, is a wheel cut with sixty sharp radial teeth, of which those corresponding to the 50th, 51st, 52d, 53d, 54th and 59th seconds of the minute have been removed by a file. Near the clock is a "repeater," the circuit through whose coils passes through a local battery, through a second clock in the computing room, and then through the standard clock. Each wire terminates in a delicate spring, close by the wheel just mentioned. While the extremities of these springs, which are shod with gold and platinum, rest in contact, the circuit is unbroken; it is opened by the minutest lifting of one from the other, and this is effected automatically by means of a ruby attached to one of them, and placed within reach of the wheel above mentioned. As each of these teeth passes, the ruby, and with it the spring, is lifted through a minute distance. (Not in practice more than one one-hundredth of an inch, and usually much less.) Once a second, therefore, the circuit is opened during a period of probably less than a twentieth of a second, and as the wheel advances a tooth with each vibration of the pendulum, the armature of the repeater is raised each second of the minute, until the 49th is completed.

Since the teeth corresponding to the next five seconds have been filed away, during those seconds the jewel is not touched, nor the circuit opened. The consequent silence of the "repeater's" beats draws attention to the fact that the end of the minute is approaching, its completion being indicated by the short pause caused by the absence of a tooth at the 59th second.

This action is repeated in every minute of the twenty-four hours without variation. The particular second is thus identified, but one minute is (so far as the action of the standard clock is concerned) not distinguished from another. To do

this is the work of the subsidiary clock in the computing room, through which the local wires are led, as has been mentioned. This subsidiary clock (made by Howard of Boston) may be called for distinction the "journeyman," and its principal office is not to give the time, but to interrupt the circuit, which it does on or near the completion of the 58th minute, closing it again about half a minute before the completion of the hour. When the circuit is opened by the journeyman, the repeater is silent for a minute and a half; when it is closed, the standard is again heard ticking on the repeater, and the ensuing short pause evidently precedes the first second of the first minute of the hour. The time is thus wholly derived from the standard clock, and is independent of any other, the journeyman having no power to control or in any way react upon the primary, and being able only to interrupt the message it sends; not to falsify them.

The mechanism for effecting the transmission of the time is essentially that already described, but more is needed to ensure against possible interruption. This may occur from several causes, prominently from oxidation of the platinum or gold contact surfaces; when the current must be interrupted while they are cleaned, if there be no other clock. To meet this contingency, a chronometer of peculiar construction was made for the observatory by Frodsham. It resembles the ordinary marine chronometer in external appearance, but contains in miniature the apparatus for breaking circuit already described, the wheels being cut so as to give the same signal of the approaching end of the minute, as the standand clock. The peculiarity consists less in this, however, than in a device by means of which it can be caused to gain or lose any fractional part of a second, or any number of seconds, without being stopped, and without any disturbance of its normal rate, except while the change is being effected. This chronometer is to replace the prime clock in the circuit, during any temporary stoppage of the latter for repair or adjustment.

The mechanism which has just been described, acts in connection with the local circuits of the observatory; one battery being employed for the sidereal clock and chronograph, and another for the mean-time standard. Any interruption of the main external circuits is shown at once, by the action of a galvanometer in each, which makes an audible and visible signal when the circuit is opened. The accessory apparatus, such as batteries, relays, switch-boards and so forth, which are used in every telegraph office, it will be superfluous to describe here in detail, but before following the operation of the electric current, outside the observatory, it will be well to speak of the method which has been adopted as likely to ensure most accuracy in the time keepers which control it.

The transit instrument in the western wing, is of four inches aperture, and with it and the chronograph, observations for time are made on every fair night of the year, except on Sunday, when if complete determinations have been made on the preceeding night, none are taken. The instrument is of sufficient power to follow the principal Nautical Almanac stars in the day, and these are used (or more rarely the sun), when the weather permits, if the usual night observations have been missed. From three to six stars are customarily taken, the azimuthal error of the instrument being found from the observations of each night, after the other corrections are applied, and the results determined from the chronograph and the sidereal clock. The mean error in the resulting determination of the sidereal clock correction, is from three to four hundredths of a second, but it cannot be assumed that that of the mean time standard is known within these limits, except at the time that the observations are freshly made.

It may be desirable to point out where the system pursued here differs from that in which a few signals are sent at stated hours, as at Greenwich. In the case of the time ball for instance, dropped daily by a clock at Greenwich mean noon, it is customary to compare the mean time clock which drops it, with the sidereal time a few minutes before twelve. If it (the operating clock), be slow, it is caused to gain, and if fast, caused to lose, an amount needed to bring it to coincidence before the automatic action gives the signal.

The time of this signal is nominally exact, but in fact involves the variations in rate of the standard clock or clocks, which are treated in the comparison as having their errors absolutely known. It is by no means meant to criticise this procedure, but to point out that as error must exist where the rates of the clocks are treated as constant in the intervals between observation, no less real accuracy is reached in the method employed here, in which (as the signals are being constantly sent) the signalling clock has no less nominal error at noon (for instance), than at any other hour.

When the sidereal clock has entered its beats upon the chronograph during the time of observation, the record is not interrupted until the mean time standard having been put into the same circuit, both clocks have automatically entered their time on the sheet together, and the break-circuit chronometer has done so also. The sheet being removed, and the breaks of the transit observer measured, the comparison of the various clocks with electric attachments are taken by measurement on the same sheet, and the others compared with the sidereal clock by noting coincidence of beats by ear. The resulting errors of all are then determined, reduced to a common epoch, and entered in a permanent record kept for the purpose in the following form,

(ΔT, δt, being the usual symbols for the respective corrections of error and rate):

Aug. 10, 1872. Time-stars { *η Herculis*, *α Camelop.*, *κ Ophiuchi*, *δ Herculis*, } A. E. F., observer.

At mean 9h		ΔT.	δt.
Sidereal clock,	+	7s·32	+1s·18
Break-circuit chronr.	+	2m·22s·18	+3s·30
Chron. 3242,	+	50s·05	+3s·11
Mean time standard	−	00s·27	+0s·46

The mean time clock is here 0s·27 fast by actual observation, but when the next comparison is made the following morning (at 21 hours), its error can usually be obtained only by comparison with another clock. If it be compared with each of the other clocks in turn, each, owing to the variations of its rate during the night, will probably give a slightly different result, but supposing all the time-keepers equally reliable, the probable error will be less, in taking the mean of the four, than by any single one.

The above corrections for error and rate having been applied to the sidereal clock; a comparison is taken with it in the morning, and the resulting time of the mean-time clock noted, on the assumption that the sidereal clock is an exact standard. The same comparison is made with each, after the respective corrections and rates have been applied, each being successively treated as an independent standard.

The results will then be entered in this form.

1872. August, 10d 21h.

Error of mean time standard,			− 0s·19	(by sidereal clock.)
"	"	"	− 0s·05	" break-circuit chron'r.
"	"	"	+ 0s·11	" chron'r 3242.
"	"	"	− 0s·04	" its own rate.

The mean or "adopted" error of the mean time standard is then $\frac{-0^s\cdot17}{4} = -\ 0^s\cdot04$.

In the absence of any more absolute criterion, the time of the standard in this instance is assumed to be four one hundredths of a second fast, and this value is adopted and treated as though it represented an error determined by direct comparison with the stars. The clock will be compared again at 9 in the evening, and when this "adopted error" exceeds 0s·25, such a change is made in the pendulum as will correct the error; not abruptly, but gradually during the ensuing twelve hours.

It is of course impracticable to stop the clock and raise or lower the adjusting screw twice daily for such minute corrections,

and many ingenious devices have been proposed for effecting the change without stopping the instrument. One of these as applied to a chronometer has already been referred to; another (employed at Greenwich) involves the use of a small bar magnet permanently attached to the pendulum, and swinging with it; and still another, the changing tension of a long spiral spring which connects the "bob" with the clock-case.

After considering many such plans, that adopted was the old one familiar to most observers, of placing weights on the top of the bob of the pendulum, and then adjusting the bob by the screw till it runs with them approximately, after which a small increment or decrement of the weights will keep the clock under control. This plan has the advantage of employing as an agent, gravity, whose effects can be reckoned on with more certainty than those of electricity, or the tension of a spring. In common with the others it has, however, as commonly employed, the defect that when changes are made daily or oftener, the *rate* of the clock cannot be ascertained, and that reliance must be placed at the times of comparison only on other clocks whose rates are undisturbed. The writer has, therefore, found it advantageous to use these weights quantitatively, by making them of a size such as to cause a gain of 1 second a day; ·01 an hour, etc. Weights representing three or four seconds are kept on the top of the bob, so that their removal will retard the clock if desired to that amount.

A record is kept in which the comparisons in the tabular form above given are entered twice daily, the amount of the weights and the consequent rate which the clock so controlled would have had with an undisturbed pendulum, being noted likewise.

The barometer and clock-case thermometer are also read twice daily for the purpose of laying down curves representing the separate effects of temperature and pressure. Another curve, whose ordinates represent the algebraic sum of the corresponding ordinates of the first two, shows the combined results of both, for comparison with still another representing the clock rates. These are chiefly useful in the occasionally long intervals of cloudy weather which occur in winter. At such times the clock rates are obtained by interpolation from the curves, and "weighted" according to the degree of dependence to be placed on each clock, before making up the final or "adopted error" of the standard. When observations are obtained daily, however, such precaution is needless.

Those who are aware of the number of patented devices for controling distant clocks by electricity, may perhaps feel surprise that so little mention has here been made of their use.

Some of these are of extreme ingenuity and much promise,

and the English patents covering such points are alone to be reckoned by scores. Plans have been submitted to the writer by which the clocks along any number of miles of road could be set right, and brought to uniform time in a few seconds, by the operator at the observatory, and these plans appear feasible. The arrangements adopted here, as the reader will observe, do not greatly differ from those employed in telegraphic determinations of longitude, and in fact a prolonged examination of very many ingenious devices for directly controling distant clocks led the writer to set them all aside, and to employ methods not differing in principle from those in use already, for purely scientific ends, in most American observatories.

Of the very numerous plans for controling distant clocks, that of Jones (now well known) appears to be the best, but even this is not quite reliable where the circuit is a long one. The clocks described have subsidiary apparatus enabling them to send controling currents on the Jones' plan, but thus far its use has been confined to the observatory. The whole work, external to the observatory, has therefore been hitherto done by the sending of signals, through which distant clocks may be *regulated*, but without employing means for their *control*, and though this is done over a very extended field, a brief description of it, under the three divisions into which it naturally falls, will suffice.

1st. The supply of time to watchmakers and jewellers. The "jewelers wire" passes through the Western Union Telegraph offices, and the stores of the principal jewelers of Pittsburgh. Beside each "regulator" is a telegraphic sounder, on which the observatory time is heard constantly ticking, and by which almost if not quite all of the clocks and watches of the city are thus at second-hand regulated. There is, in this uniform and recognized standard, everywhere accessible, a convenience to watchmakers of course, but still more to the public, as the discrepancies between clocks, public or private, which cause so many lost minutes in the day to each person in a city, that their aggregate represents a large draft upon the time of the business public, disappear.

Applications have been received from watchmakers in neighboring cities and at a considerable distance from Pittsburgh, for this telegraphic supply of the time, which it has not always been possible to accommodate, but which have been welcome as showing a public appreciation of the utility of the work.

2d. The supply of time to railroads. The watchmakers and jewelers are in permanent telegraphic connection with the observatory by a wire which is devoted to their use, but dis-

tant cities, such as Chicago or Philadelphia, can be reached only by the wires of the telegraph or railroad companies, which are too valuable to be exclusively employed for this purpose. The method used on the Pennsylvania Central, and Pittsburgh, Fort Wayne and Chicago roads, will sufficiently illustrate the system as applied to railways.

A special wire connects the observatory with the office in which the wires owned by these roads unite. In this office is a small bell which is struck lightly every second, in the manner described, and except for the pauses to designate the minute and hour, continues to sound unintermittingly; affording to the conductors and other employees specially concerned in the time a means of ready comparison, even without entering the building.

At 9 and at 4, Altoona time (ten minutes fast of Pittsburgh), the Pittsburgh operator in charge connects the main eastern wire to Philadelphia, 354 miles distant, with the observatory, and for the ensuing five minutes the beats of the Howard mean-time standard are automatically repeated on similar bells, or on the customary "sounders" in Philadelphia and in every telegraph office through which the road wire passes; all station clocks and conductor's watches being compared with it as the official standard. After five minutes the clock is "switched" by the Pittsburgh operator out of the main line wire, which is returned to its ordinary use.

A similar set of signals, lasting for five minutes, is sent at 9 and 4 of Columbus time (13 minutes slow of Pittsburgh) to all stations as far west as Chicago inclusive, in the main western line (of 468 miles in length). At Philadelphia the time is repeated to New York, at Harrisburgh to Erie (333 miles), etc. As it is thus sent not only over the main lines from New York to Chicago (nearly a thousand miles), but over a number of subsidiary or branch roads too great for enumeration here, and which form in the aggregate a much larger number of miles than the main trunk, it will be observed that a considerable fraction of the railway system of the whole country is prepared for using a single unit of time, as, though the names of "Philadelphia time," "Altoona" or "Columbus time" are not yet abolished over that part of our railway system referred to, every railroad clock and watch, and the movement of every train, is regulated from a single standard, that of the clock in the observatory.

The advantages of this uniform and wide distribution of exact time in facilitating the transportation of the country, and in enhancing the safety of life and of merchandize in transit between the Western and the Atlantic cities seem to be sufficiently evident. The fact that the system, described in this article, has obtained the extension it has, within three years

from its commencement will, it may be hoped, justify the belief that its use has proved not only valuable to railways, but an added security to the safety of the public.

3d. Supply of time to cities. At present, arrangements are in progress for regulating the principal public clock of Pittsburg, (the turret clock of the City Hall, about two miles from the observatory), which it is intended shall strike every third hour on the bells of the fire alarm, and probably also in the various police stations. As the mechanism for doing this is still in course of construction, and may yet be modified in trial, it would be premature to speak of it, especially as its success has not yet been proven in practice here. The city clock will automatically report its own time to the observatory by a special wire, and it is probable that in controling its rate from the observatory, the "Jones" system will be used.

The necessity of a uniform standard of time over the whole country, which was alluded to in the outset as one of growing importance, has not been further directly touched upon in this article, which is yet as a whole devoted to describing the means of meeting it. The evident tendency, in thus sending the time from one standard over so large an extent of territory, is to diminish the number of local times, and so prepare the way for a future system, in which, at least between the Atlantic and the Mississippi, they shall disappear altogether.

A step in this direction has been contemplated by the managers of the roads uniting New York, Philadelphia, Pittsburgh and Chicago, who have intended to use the time of the meridian of Pittsburg between the two extreme points mentioned, running all trains from New York to Chicago by this time alone, in place of using successively the local times of Philadelphia, Altoona and Columbus, as at present. Such a change would have already taken place during the last summer, except for an unexpected cause of delay, on whose removal it will be effected.

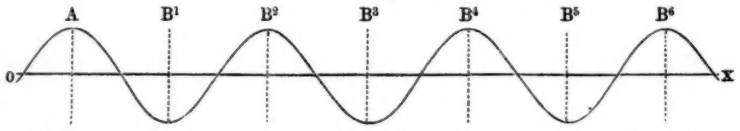
The labors of this and of other American Observatories are tending to the same important end, that of the ultimate adoption of some single time for all the country east of the Mississippi, by which not only the railroads, but cities and the public generally, will regulate themselves. What point shall be chosen is of less importance than that some *one* shall be used and universally.

The subject is one which has hitherto attracted little public attention, but it does not seem unsafe to make the assertion, that the causes which have almost insensibly effected such a revolution in England, will in a few years more bring it about here.

Allegheny Observatory, Allegheny, Penn., Sept. 22, 1872.

ART. XLV.—*On a method of detecting the phases of vibration in the air surrounding a sounding body; and thereby measuring directly in the vibrating air the length of its waves and exploring the form of its wave-surface;* by ALFRED M. MAYER, Ph.D., Member of the National Academy of Sciences. Professor of Physics in the Stevens Institute of Technology.

THE curve A, B^2, B^4, etc., is the well known symbolic representation of the dynamic condition of the air, at a given instant, when traversed by simple sonorous vibrations. Those portions of the curve above the axis OX represent the length and manner



of the aerial condensations, while those flexures below the axis represent the rarefactions; therefore similar points of the flexures above the axis, or similar points in the flexures below the axis, represent like phases of vibratory motion. Imagine these conditions of the air produced by a body vibrating at A; then the distance A to B^1, B^1 to B^2, etc., will equal a half wave-length, while from A to B^2, B^2 to B^4, etc., will represent a whole wave-length, corresponding to the note given by A. If another sonorous body B, giving exactly the same note as A, be placed anywhere on OX, it will have vibrations communicated to it from the vibrating air which touches it, and it will vibrate exactly with the air, almost as though its substance was of the air itself. Now imagine this body B placed at B^2, or at B^4, or B^6, etc., then its phases of vibration will be exactly similar to those of A; but when placed at B^1, B^3 or B^5, etc., its phases of vibration will be opposed to those of A. That is, at distances from A, equal to any number of whole wave-lengths the body B will, at the same moment of time, swing with A, but at distances from A equal to any number of half wave-lengths the direction of its swings, at any given instant, will be opposed to A; while at intermediate positions, on the line OX, the oscillations of B will be lagging somewhat behind or be slightly in advance of the phase of A's vibration.

After this it is evident that if, by any means, we can *see at the same time* the vibrations of A and of B, we will (if the received conception of the nature of a vibration's propagation is correct) see their motions just as has been described above, and will therefore be able to measure, *directly in the air*, a wave-length and to determine any wave surface enclosing a vibrating body. I have devised several processes. I will, however, here

describe only two; the first, though impracticable, I speak of to render clear the general method of all; the second I give on account of its simplicity, ease of execution, and the superior accuracy of its numerical results.

Take two tuning forks giving the same note and having mirrors attached to their similar prongs; place one at A, the other anywhere on the line OX. Reflect a pencil of light from each mirror of the forks to a revolving mirror, whose axis of rotation is in a plane parallel to the planes of vibration of the forks. If the fork B, which vibrates sympathetically, be placed at B^2, B^4, B^6, etc, then the two pencils reflected from the forks will, on striking the revolving mirror, be drawn into two sinuous curves, and the flexures of the two curves will run parallel to each other, that is, the curves will appear as the two rails of a sinuous railway; but, if the fork B be placed at B^1, B^3, or B^5, etc., then the sinuosities of the two curves will no longer be parallel but will be opposed; that is, where a flexure of one of the curves is concave on the left, the opposite flexure of the other curve will have its concavity on the right. If the fork B be placed at intermediate positions, in reference to those above stated, we will have neither concordance nor opposition of the flexures, but intermediate relations depending on the fraction of half wave-lengths at which the sympathetic fork is placed on the line OX.

It is now readily seen that if we should place the fork B at two successive points, as B^2 and B^4, on the line OX, so that exact concordance of flexures of the curves should be seen at each of these points, then evidently we have placed the fork at two positions removed from each other by a wave-length, for at these points the air had at the same instant the same phase of vibration. Thus we have measured a wave-length. Furthermore, if by any means we could move the fork B around A so that during this motion it always preserved, in reference to A, the same relation of vibratory phase, we would have determined the form of the wave-surface produced by the propagation of A's vibrations.

The above is an exposition of the thoughts that have occupied my mind for several months, and they ultimately led to the following method, by which all I have narrated can be accomplished without any difficulty; thanks to the inventive genius of Mr. König, to whose skillful aid so many physicists are continually indebted.

The membranes of Mr. König's manometric capsules furnish us with surfaces which vibrate in perfect accordance with the air which touches them, and we can lead the impulses of these membranes through gum tubes to gas jets placed at any desired point, where the vibrations of their flames can be compared. Thus they are far superior to the tuning forks, which require

the relations of delicate adjustments to be maintained during each change of position, and therefore forks could only with difficulty be made to serve in the measure of a wave-length, and could not at all be employed to trace out a wave-surface on account of the impossibility of a continuous comparison of their vibrations, which latter condition the manometric flames admirably fulfill.

The Experiments.

Let us now proceed to experiment. I placed on the acoustic bellows an open UT_3 organ pipe, and from its ventral manometric capsule I led a tube to a gas jet placed in front of a cubical revolving mirror. I took an UT_3 Helmholtz resonator and adapted to its beak a gum tube, with an interior diameter of 1 centimeter and a length of over 4 meters. This tube led to a firmly supported manometric capsule whose flame was placed quite close to and directly behind, the organ pipe flame, which latter had about twice the height of the resonator flame. On sounding the pipe and holding the resonator quite near it, the two flames, by a slight adjustment, were made to appear as *one series of serrations* in the revolving mirror. Now on gradually moving the resonator away from the pipe, I saw another series of serrations (those of the resonator flame) slowly evolve themselves from the first series, and gradually slide over the latter, until, having removed the resonator from its first position by about 66 centimeters or a half wave-length (German), I had the pleasure of seeing the series of moving serrations standing exactly midway between the first or immovable series. On moving the resonator yet farther from the sounding pipe, I saw the serrations of the resonator flame continue their onward progress until the two series again coincided; and on measuring the distance of the resonator from its first position near the pipe, I found it to be equal to a whole wave-length of the note UT_3. When I had removed the resonator one and a half wave-length, I again saw the serrations of the resonator flame bisecting the spaces between the serrations given by the organ pipe flame, and when the resonator had progressed from the pipe to a distance equal to two whole wave-lengths I saw that the serrations of its flame had progressed to another coincidence with those of the organ pipe; and so on, until I had determined on the line of the resonator's motion *all the phases of vibration* corresponding to three whole wave-lengths. I now moved the resonator until I had again caused the serrations of its flame neatly to bisect the spaces between the serrations of the organ pipe flame, and moving around the organ pipe, with the resonator held at such distances from it that the bisections were steadily kept, I described in space the wave surface of the sounding pipe. This surface I found approximately to be an ellipsoid with its foci

at the top and bottom of the pipe. Nothing could be more satisfactory, and it was charming to behold how neatly the surface could be determined; for a small change in the distance of the resonator from the pipe produced a sensible shifting of the serrations. I now substituted for the resonator an organ pipe, in every respect similar to the one on the bellows, and with it I repeated the wave-length measures previously made with the resonator; indeed the column of air in the pipe in my hand responded so perfectly to the sounding pipe that I thought it gave more marked results than those produced with the resonator.

The manometric flame-micrometer.

In the experiments described above, we examined the appearances in the mirror with the unaided eye, and with it estimated when coincidences and bisections occurred; but to obtain results of precision, a method was devised which determines neatly these critical points. For that purpose I have invented the following micrometer, founded on a beautiful suggestion of Dr. R. Radau, who thus describes in his excellent "*l'Acoustique*" (Paris, 1867, p. 272), a method of observing the flames of two similar sounding organ pipes. "We attach to the two pipes two of König's flames arranged so that the point of one flame reaches above a small fixed mirror which hides its base, but which shows by reflection the base of the other flame. This produces the illusion of a single flame. If now we observe this hybrid image in the revolving mirror while we sound the two pipes, the point separates from the base, which proves that the two flames shine *alternately*, and the one retracts while the other elongates; if the two tubes act on the same flame, the effect is null, and the flame remains immovable." By placing the above "small fixed mirror" on a divided circle; or by silvering its back and determining its angular displacements around a vertical axis by the method of Poggendorff,—that is, by observing through a telescope, the reflections of a fixed scale from the back of the mirror,—we have devised a simple and precise micrometer for ascertaining the amount of displacement of the resonator's flame. For, having once determined, for a given note, the amount of angular motion of the mirror required to move the bases of the flames over the distance between the centers of two contiguous serrations we have the angular value of a displacement equal to that caused by moving the resonator through a wave-length, and a fraction of the turn required to produce the above movement of the bases of the flames will be equal to that produced by the remove of the resonator over a corresponding fraction of a wave-length. Thus can be measured very small fractions of a wave-length. Indeed, even with the unaided eye and without the use of the micrometric

mirror I have distinctly detected a displacement of the flames on moving the resonator, (UT_3) over only 3 centimeters or $\frac{1}{44}$th of a wave-length, and with the micrometer I feel assured that I can determine the wave-surface of a body giving the note UT_3 to one centimeter of its true position. Of course with higher notes we shall get a proportionally closer determination. But the object of this paper is not to present numerical results; I reserve these for a subsequent communication, in which I will also present diagrams of apparatus and the appearances of the flames in various experiments.

I will here remark that the success of the experiments depends on the resonator with its attached tube being in perfect unison with the organ pipe; also, the relative heights and positions of the flames should be so adjusted that the sharpest definitions are obtained in the rotating mirror, and thus be able to detect and measure the effects of small changes in the position of the resonator; but these and other manipulative details will readily occur to any physicist who repeats the experiments.

Applications of the Method.

When the method I have here blocked out shall have been reduced to the refinement which it is susceptible of, I feel confident that we will have in our hands the power to attack many problems of high theoretic interest which have heretofore been deemed beyond the reach of experiment. Its application to such are so numerous that they are almost co-extensive with the phenomena of sound.

The actual experimental determination of wave-surfaces in free air and in buildings can now certainly be accomplished; and such determinations may serve to extend our knowledge in the direction of giving the proper laws which should govern architects in their construction of rooms for public assemblies.

The differences, if any, in the velocities of sound, corresponding to vibrations differing in intensities and frequencies, may be determined by the use of reflectors, and the direct observation on any changes in wave-lengths different from those which should be given on the assumption that notes of various intensities and heights are propagated with the same velocity.

We can determine a wave-length quite accurately by the following arrangement of apparatus. Take an organ pipe with a resonator in unison with it, and place the resonator in a fixed position opposite the mouth of the pipe, then lead tubes from the capsules of pipe and resonator to contiguous jets, and adjust their flames to coincidence or to bisection of serrations; using for this purpose the manometric micrometer. Now suppose, for simplicity, that the pipe gives 340 complete vibrations in a second; then, as the velocity of sound is 340 meters per second, it is evident that in $\frac{1}{340}$th of a second an aerial pulse will traverse one meter. Therefore, if all things else remain the

same, and we lengthen the resonator tube $\frac{1}{2}$ meter, the serrated flames of the resonator will be displaced $\frac{1}{2}$ of the distance between the centers of two contiguous serrations; and if the tube be lengthened 1 meter, or *one wave-length*, the displacement will amount to the entire distance separating the centers of two contiguous serrations: and for n number of wave-lengths of elongation of tube, we shall have n number of such displacements. Thus can be measured a wave-length; and if the number of vibrations given by the pipe be accurately known, we can reach with the manometric micrometer an accurate determination of the velocity of sound.

Finally, we are bold enough to believe that we have in the highest development of the method, a means of tracking in the air the resultant wave-surface of combined notes; and, in short, of bringing the exploration of acoustic space to approach somewhat to that precision of measurement which, for over half a century, has characterized the study of the æthereal vibrations producing light.

September 21st, 1872.

ART. XLVI.—*Growth or Evolution of Structure in Seedlings;* by JOHN C. DRAPER, M.D.

THE continuous absorption of oxygen, and formation of carbonic acid, is an essential condition of evolution of structure, both in plants and in animals.

The above proposition in so far as it relates to animals will probably be admitted by all; the opposite opinion is, however, commonly held as regards plants; yet we propose to show that in these organisms, as in animals, growth as applied to evolution of structure, or organization of material provided is inseparably connected with oxidation.

The discussion of the proposition in question necessarily involves a preliminary review of the character of the gases exhaled from various plants. Commencing with the lower organisms as fungi, the uniform testimony is that these plants at all times expire carbonic acid, while it is chiefly in the higher plants, and especially in those which contain chlorophyl or green coloring matter that carbonic acid is absorbed and oxygen exhaled. The inquiry then in reality narrows itself down to the examination of the growth of chlorophyl-bearing plants.

Regarding these plants the statement is made and received, that they change their action according as they are examined in the light or in the dark, exhaling oxygen under the first condition, and carbonic acid under the second. Various explanations of this change of action have been given, that generally accepted accounting for it on the hypothesis of the absorption

of carbonic acid by the roots, and its exhalation by the leaves when light is no longer present.

The change, on the contrary, appears to arise out of the fact that two essentially different operations have been confounded, viz: the actual growth or evolution of structures in the plant, and the decomposition of carbonic acid by the leaves under the influence of the light, to provide the gum or other materials that are to be organized. These two factors are separated by Prof. J. W. Draper in his disscusion of the conditions of growth in plants. We propose to show that by adopting this proposition of two distinct operations in the higher plants, all the apparent discrepancies regarding the growth of these plants are explained.

The growth of seedlings in the dark offering conditions in which the act of growth or evolution of structure is accomplished without the collateral decomposition of carbonic acid, I arranged two series of experiments in which growth under this condition might be studied and compared with a similar growth in the light. That the experiments might continue over a sufficient period of time to furnish reliable comparative results, I selected peas as the subject of trial, since these seeds contain sufficient material to support the growth of seedlings for a couple of weeks.

To secure as far as possible uniformity of conditions between the dark and light series, and also to facilitate the separation, cleansing and weighing of the roots, each pea was planted in a glass cylinder, one inch in diameter and six inches long. These cylinders were loosely closed below by a cork, and filled to within half an inch of the top with fine earth or vegetable mould. They were then placed erect in a covered tin box or tube-stand, in such a manner that the lower end dipped into water contained in the box, while the whole of the cylinder except the top was kept in the dark. Thus the first condition for germination, viz., darkness, was secured; the second, warmth, was supplied by the external temperature, which varied from 70° to 80° F., while regularity and uniformity in the supply of moisture in both series was secured by having a box of cylinders or tubes for each and keeping the level of the water the same in both. The supply of oxygen was also equal and uniform, since the upper part of each tube presented a similar opening to the air.

Thus prepared, one box containing five cylinders was kept in a dark closet, while a second similar in all respects was placed in a window of the adjoining room, where it was exposed to direct sunlight five or six hours every day. To each tube a light wooden rod thirty inches in length was attached, and on this the growth of the seedling was marked every twelve hours.

The hours selected were 7 A. M. and 7 P. M. I thus obtained the night and day, or dark and light growth of every seedling, as long as those in the dark grew. The seeds were planted on June 1st, and appeared above the ground on June 6th, when the measurements were commenced. In each series one seed failed to germinate; the record consequently is for four plants in each, and the history of the evolution of structures is as follows:

Evolution of structure in the dark.—In Table I. the seeds are designated as A, B, C, D, and each column shows the date on which leaves and lateral growths appeared. These constitute periods in the development of the plants, which are indicated by the numbers 1, 2, 3, 4, 5, 6. The weight of each seed is given in milligrammes.

Table I.—*Seedlings grown in the dark.*

Weight of seed.		A. 431.	B. 436.	C. 456.	D. 500.
Period	1,	7th day.	7th day.	7th day.	7th day.
"	2,	8th "	9th "	9th "	8th "
"	3,	10th "	10th "	11th "	10th "
"	4,	12th "	12th "	13th "	12th "
"	5,	14th "	15th "	15th "	14th "
"	6,	17th "	18th "	18th "	17th "

A glance at the above shows the uniformity as regards time with which the structures were evolved in each plant. It also indicates for each plant an equality in the number of periods of evolution, viz., 6, notwithstanding the difference in the weights of the seeds; and suggests that the power of evolution of structure in seedlings resides in the germ alone.

The character of the evolution in the six periods shows a steady improvement or progression.

In the first, the growth consists of the formation close to the stem of two partially developed pale yellow leaves.

The second period is similar to the first, except that the leaves are a little larger.

The third presents a pair of small yellow leaves close to the main stem, from between which a lateral stem or twig about one inch long projects, and bears at its extremity a second pair of imperfectly developed yellow leaves, from between which a small tendril about a sixteenth of an inch long is given off.

The fourth resembles the third, the lateral twig being longer, and the tendril three times as long as in the third.

The fifth is like the fourth, except that the tendril bifurcates.

The sixth is similar to the fifth, except that the tendril trifurcates.

Stem, leaves, twigs, tendrils of various degrees of complexity, all are evolved by the force pre-existing in the germ without the assistance of light.

Evolution of structure in the light.

Table II.—*Seedlings grown in the light.*

Weight of seed.	E. 288.	F. 426.	G. 462.	H. 544.
Period 1,	—	6th day.	—	6th day.
" 2,	7th day.	7th "	7th day.	7th "
" 3,	8th "	8th "	8th "	9th "
" 4,	12th "	9th "	10th "	10th "
" 5,	15th "	11th "	14th "	12th "
" 6,	— "	13th "	— "	14th "

Table II. was obtained in the same manner as Table I, the columns representing the days on which lateral growths and leaves appeared. Though there is not the same uniformity as in Table I, the periods are identical in both as regards the visible character of the evolution. Nothing appears in the second that did not pre-exist in the first, and in the case of the seeds E and G the evolution is even deficient as regards the first and the sixth periods.

While the general character of the evolution in both series is similar, certain minor differences exist. In II. the leaves and tendrils are many times larger than in I, and they with the whole plant are of a bright green color, instead of the sickly pale yellow of I; but the light has not developed any new structure; it has only perfected those which pre-existed, and converted other substances into chlorophyl which is not an organized body.

Not only did the plants in the two series present similarities in evolution of structure, but the average weight of dry plant in each was very nearly the same, for:

455 of seeds in the dark produced 184 of dry plant, while
455 " light " 215 " "

A comparison of the parts below the ground with those above (both being dried at 212° F.) shows that the proportion of root to total weight of plant was also nearly identical; being,

25 of root for 100 of plant in the dark, and
23 " 100 " light.

The close similarity in the evolution of visible structure in the light and in the dark, the small difference in the total weights of the plants grown in the same time in both series, and the close approximation in the proportional weight of root to plant, all justify the conclusion, that the growth in darkness and in light closely resemble each other, and that it is proper to reason as regards the nature of the action from the first to the second.

Another interesting fact which lends support to the opinion that the process of growth in seedlings developed in the dark

is very similar to that occurring in those grown in the light, is the character of the excrements thrown out by the roots. It is well known that many plants so poison the soil that the same plants cannot be made to grow therein until the poisonous excretions from the roots of the first crop have been destroyed by oxidation. In the case of peas this poisoning of the soil takes place in a very marked manner, and I have found that in the pots in which peas have been grown in the dark, the soil is so poisoned by the excrements from the roots that a second crop fails to sprout. Does it not follow, that since in the two series with which I experimented, the excrements from the roots possessed the same poisoning action, the processes in the plants from which these excrements arose must have been similar?

There remains an important argument concerning which nothing has thus far been said. It is to be derived from the consideration of the rate of growth in the light series during various periods of the day of twenty-four hours. If the evolution of structure in a plant in daylight is the result of the action of light, that evolution should occur entirely, or almost entirely during the day. If on the contrary it is independent of the light, it should go on at a uniform rate as in plants in the dark.

For the elucidation of this portion of the subject, I present the following tables; the first of which shows the growth by night, 7 P. M. to 7 A. M. of the seedlings in the dark series, compared with their growth by day, 7 A. M. to 7 P. M. The measurements were taken from the sixth to the twentieth of the month, the day on which growth ceased in the dark series.

Table III.—*Seedlings grown in the dark.*

	Night growth.		Day growth.	
No. 1	$12\frac{3}{4}$	inches.	14	inches.
No. 2	$13\frac{1}{4}$	"	13	"
No. 3	$11\frac{3}{4}$	"	$11\frac{3}{8}$	"
No. 4	$12\frac{5}{8}$	"	$11\frac{3}{8}$	"
Average,	$12\frac{5}{8}$	"	Average, $12\frac{3}{8}$	"

The total day growth and night growth under these circumstances are nearly equal, though there is a slight excess in favor of the night, amounting as the table shows, to $\frac{2}{8}$ of an inch in 12 inches.

In Table IV. the growth of the light series is given in the same manner, by day and by night, for the same time, viz: to June 20th. The thermometric and hygrometric conditions in both series were very similar, as indicated by the dry and wet bulb thermometers suspended in the vicinity of each set of tubes.

Table IV.—*Seedlings grown in the light.*

	Night growth.	Day growth.
No. 5	3¼ inches.	4 inches.
No. 6	8 "	7 "
No. 7	5¼ "	4½ "
No. 8	9½ "	8½ "
	Average, 6½ "	Average, 6 "

In the average, and throughout the table, with a single exception, not only is the uniformity in the rate of growth during the day and night shown, but the slight excess of night growth found in the series kept in the dark is likewise copied. We must therefore accept the conclusion, that the act of growth or evolution of structure is independent of light, and that the manner of growth during the day is similar to that at night.

It will be noticed that the total average height attained in the light is only about half that in the dark series. The explanation of this we have already seen in the fact, that in the former the leaves and tendrils were much larger than in the latter, while the dry weights were nearly the same. The material of the seed in the light series was consumed in extending these surfaces, while in the dark series it was spent in lengthening the stem.

Having established the continuous character of growth in seedlings, and the similarity of rate and nature of the process by night and by day, and admitting that at night plants throw off carbonic acid, it is not improbable that this carbonic acid arises, not from mechanical absorption by the roots, and vaporization by the leaves, but as a direct result or concomitant of the act or process of evolution of structure.

To put the matter in the clearest form, let us first understand what growth is. It appears in all cases to consist in the evolution or production of cells from those already existing. According as the circumstances under which the cells are produced vary, so does the tissue ultimately produced vary. Cells formed in woody fibre become wood. Cells formed in muscle in their turn form muscles, but the starting point of the process in every instance is the formation of new cells.

If now we examine the evolution of cells under the simplest conditions, as for example in the fermentation that attends the manufacture of alcohol, we find that with the evolution of the torulæ cells carbonic acid is produced. The two results are intimately connected, and it is proper to suppose that since the carbonic acid has arisen along with the new cells, the latter operation must in some way involve a process of oxidation. Accepting the hypothesis that oxidation is attendant on these processes of cell growth under the simplest conditions, we pass

to the examination of what occurs in the lower forms of vegetable organisms found in the air.

The Fungi, and indeed all plants that are not green, with a few exceptions, exhale carbonic acid and never exhale oxygen. In this case, in which cell production often occurs with such marvelous rapidity, the carbonic acid must have arisen as a consequent of the cell growth. It is improbable that it has been absorbed by roots and exhaled from the structures, either in these plants or in those produced during fermentation. In the latter there never are any roots, and in the former, even where roots are present, they bear a small proportion to the whole plant. The quantity of moisture exhaled by such growths is also insignificant, and out of proportion to the carbonic acid evolved. We must, therefore, in this case decline to accept the root absorption hypothesis, and admit that the carbonic acid has arisen as a result of the cell growth in the plant.

Passing to the chlorophyl-bearing plants, we find that in the Phanerogamia it is only the green parts that at any time exhale oxygen, and then only under the influence of sunshine. The other parts of the plant above the ground, that are not green, viz., the stem, twigs, flowers, etc., are at all times, day and night, exhaling carbonic acid. The whole history of the plant, from the time the seed is planted to its death, is a continuous story of oxidation, *except when sunlight is falling on the leaves.* The seed is put into the ground and during germination oxygen is absorbed and carbonic acid exhaled. If the seedling is kept in the dark, oxygen is never exhaled, only carbonic acid, and the plant not only grows, but all visible structures except flowers are formed in a rudimentary condition. In the light the growth during the night time is attended by the evolution of carbonic acid, while during the day time the bark of the stem and branches is throwing off carbonic acid. When flowers and seeds form, the evolution of carbonic acid attending this highest act of which the plant is capable, is often greater than that produced at any time in many animals.

Everything in the history of plants, therefore, tends to show that the evolution of their structures is inseparably attended by the formation of carbonic acid, and it seems impossible, when we consider the evolution alone, to arrive at any other opinion than that already expressed—that, *all living things, whether plant or animal, absorb oxygen and evolve carbonic acid, or some other oxidized substance, as an essential condition of the evolution of their structures.*

College of the City of New York, Sept. 12th, 1872.

ART. XLVII.—*Rejoinder to Prof. Hall's Reply to a "Note on a question of Priority"*; by E. BILLINGS.

To answer the whole of Professor Hall's reply, published in the August number of this Journal, would require a long article. I beg, however, to notice briefly several of the leading points.

With regard to publication, I hold it to be the duty of an author who describes new fossils to make his work accessible to the public. If he fail to do this, he cannot claim priority over one who has published in the regular way. His work may be adopted as a matter of courtesy, but not to take precedence over fair publication. Prof. Hall's pamphlet was not accessible to the public at the time my paper was published, and therefore his genus *Rhynobolus* cannot take priority over my genus *Obolellina*. During the discussion that has taken place it has been argued, with reference to publication, that "no determined rules or laws have been hitherto settled or followed." On this point, I hold that there are laws which result spontaneously from the very nature of the circumstances to which they relate. These laws exist perpetually, although not instituted by legislative enactment, and although they may be habitually transgressed by any number of unscrupulous persons. The law of publication is one of these. Every true naturalist instinctively feels and knows that such a law does exist, and that it is his duty to observe it. My genus was fairly published, in accordance with the requirements of this law, and therefore it must stand, unless, by some miracle, the law should cease to exist.

The genus *Obolellina* belongs to the *Trimerella* group of fossils. Prof. Hall borrowed some of our specimens of that group, and notice was given him that I was at work upon them. The object of giving this notice was to prevent any occurrence which might lead to ill feeling. Instead of doing his best to carry out this object, he almost immediately proposed his genus *Rhynobolus*, on a Canadian specimen from the same locality where those lent him had been collected. I believe he is the only paleontologist in America who would have taken such a course. He attempts to justify himself by saying that I had procured specimens from New York. But the case is wholly different. I require New York fossils for comparison, and for that purpose have bought them, collected them myself, and sent others to collect them for me. But I only use them for comparison. I never described a new species collected in New York. On the other hand, Prof. Hall collects Canadian fossils, and goes further. He describes the new species. He

even visited a party who, as he well knew, was collecting for our survey, and procured a collection from him. In his reply, he gives the reader to understand that he has refrained from describing Canadian species "*from a natural sense of propriety.*" Where was this sense of propriety when he described the fossils from Cayuga, Canada West, in vol. iv, Pal. N.Y.; for instance, *Cryptonella iphis*, *Centronella ovata*, and *Meristella lenta?*

He intimates that "in nearly all cases" where he has proposed new genera, during the last "ten or fifteen years," I have raised an objection of some kind. Since reading his paper I have looked over his works, and have made a list of ninety-seven genera proposed by him during the period mentioned. Of these, I have objected to not more than a dozen. His statement is therefore a gross exaggeration. I have not made that sweeping condemnation of his work that he would desire to be imputed to me.

There is, besides the above, nothing in his reply but matter totally irrelative to the subject in dispute.

ART. XLVIII.—*Elements of Planets* (122) *and* (123); by Prof. C. H. F. PETERS. From a letter to one of the editors, dated Litchfield Observatory of Hamilton College, Clinton, Oneida Co., N. Y., October 15, 1872.

THE planets (122) and (123), of which the former has received the name of *Gerda* (from Scandinavian mythology), the latter that of *Brunhilda* (well known from the Nibelungen), have now gone out of sight, or become so faint that further observations would be of little value. I have, consequently, computed their orbits, for each selecting from the series of observations three positions suitably distributed. The elements, which, on account of the length of the area employed, may be assumed to possess already a great degree of reliability, result as follows:

(122) *Gerda*, from obs. Aug. 1, Aug. 28, and Sept. 24.

Epoch: 1872, August 28·0, Berlin mean time.

$M_0 = 112° 59' 34''.63$ $\varphi = 2° 5' 23''.74$

$\pi = 208 \ 12 \ 7.76$ $\mu = 613''.5218$

$\Omega = 178 \ 56 \ 41.89$ Mean eq. 1872·0 $\log a = 0.5081178$

$i = 1 \ 36 \ 16.85$

(123) *Brunhilda*, from obs. Aug. 1, Aug. 27, and Sept. 25.

Epoch: 1872, August 27·0, Berlin mean time.

$M_0 = 267° 54' 28''.70$ $\varphi = 6° 30' 23''.32$

$\pi = 71 \ 51 \ 32.93$ $\mu = 803''.1187$

$\Omega = 308 \ 42 \ 13.02$ Mean eq. 1872·0 $\log a = 0.4301512$

$i = 6 \ 28 \ 32.48$

The orbit of *Gerda* is remarkable for having both the inclination and eccentricity very small, a coincidence which is not found among the other known asteroids except in the orbit of *Clytia*.

Upon re-computing, for a check, the observations from the elements, the following insignificant differences remained (calc.—obs.):

	(122)			(123)	
	$\Delta\alpha$	$\Delta\delta$		$\Delta\alpha$	$\Delta\delta$
Aug. 1	$-0^s{\cdot}01$	$-0''{\cdot}1$	Aug. 1	$-0^s{\cdot}01$	$-0''{\cdot}1$
Aug. 28	$-0{\cdot}02$	$0{\cdot}0$	Aug. 27	$-0{\cdot}04$	$-0{\cdot}2$
Sept. 24	$-0{\cdot}04$	$-0{\cdot}1$	Sept. 22	$-0{\cdot}03$	$0{\cdot}0$

The planet (124) (named *Abeste*) a few days ago reached its stationary point, so that now its right ascension is increasing. Its brightness, now about 11·2 magnitude, will permit observations for some time yet.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the Chemical Efficiency of Sunlight;* by James Dewar, Esq. (Phil. Mag., Oct. 1872.)—Of all the processes proposed to measure varying luminous intensity by means of chemical effects, not one has yet been expressed in strictly dynamical measure. This is owing to the very small amount of energy to be measured necessitating very peculiar processes for its recognition. The chemical actions generally induced by light are of the "Trigger" or "Relay" description—that is, bear no necessary relation to the power envolved by the transformation. There is one natural action of light, however, of a very different kind, continuously at work in the decomposition of carbonic acid by plants, necessitating a large absorption of energy, and thus enabling us to ascertain the proportion of the radiant power retained, through the chemical syntheses affected.

So far as I am aware, the following passage, extracted from Helmholtz's Lectures "On the Conservation of Energy," delivered at the Royal Institution in 1864, and published in the Medical Times and Gazette, contains the first estimate of the chemical efficiency of sunlight. "Now, we have seen already, that by the life of plants great stores of energy are collected in the form of combustible matter, and that they are collected under the influence of solar light. I have shown you in the last lecture that some parts of solar light—the so-called chemical rays, the blue and the violet which produce chemical action—are completely absorbed and taken away by the green leaves of plants; and we must suppose that these chemical rays afford that amount of

energy which is necessary to decompose again the carbonic acid and water in their elements, to separate the oxygen, to give it back to the atmosphere, and to collect the carbon and hydrogen of the water and carbonic acid in the body of the plant itself. It is not yet possible to show that there exists an accurate equivalent proportion between the power or energy of the solar rays which are absorbed by the green leaves of plants, and the energy which is stored up in the form of chemical force in the interior of the plants. We are not yet able to make so accurate a measurement of both these stores of energy as to be able to show that there is an equivalent proportion. We can only show that the amount of energy which the rays of the sun bring to the work is completely sufficient to produce such an effect as this chemical effect going on in the plant. I will give you some figures in reference to this. It is found in a piece of cultivated land producing corn or trees; one may reckon per year and per square foot of land 0·036 lb. of carbon to be produced by vegetation. This is the amount of carbon which during one year, on the surface of a square foot in our latitude, can be produced under the influence of solar rays. This quantity, when used as fuel and burnt to produce carbonic acid, gives so much heat that 291 lbs. of water could be heated 1° C. Now we know the whole quantity of solar light which comes down to one square foot of terrestrial surface during one second, or one minute, or one year. The whole amount which comes down during a year to one square foot is sufficient to raise the temperature of 430,000 lbs. of water 1° C. The amount of heat which can be produced by fuel growing upon one square foot during one year is, as you see from these figures, a very small fraction of the whole amount of solar heat which can be produced by the solar rays. It is only the 1477th part of the whole energy of solar light. It is impossible to determine the quantity of solar heat so accurately that we could detect the loss of so small a fraction as is absorbed by plants and converted into other forms of energy. Therefore, at present, we can only show that the amount of solar heat is sufficient to produce the effects of vegetable life, but we cannot yet prove that this is a complete equivalent ratio." This estimate is, strictly speaking, the mean agricultural efficiency of a given area of land, cultivated as forest; and considering that active growth only takes place during five months in the year, we may safely adopt $\frac{1}{600}$ of the total energy of sunlight as a fair value of the conserved power, on a given area of the earth's surface in this latitude during the course of the summer. As chlorophyl in one or other of its forms is the substance through which light becomes absorbed and chemical decomposition ensues, it would be interesting to acquire some idea of the storage of power effected by a given erea of leaf-surface during the course of a day, and to compare this with the total available energy. Here we are dealing with strictly measureable quantities, provided we could determine the equation of chemical trans formation.

Boussingault's recent observations on the amount of carbonic acid decomposed by a given area of green leaf seem to me to afford interesting data for a new determination of the efficiency of sunlight. By experiments made between the months of January and October under the most favorable circumstances in atmospheres rich in CO^2, one square decimetre of leaf was found to decompose in one hour, as a mean, 5·28 cub. centims. of CO^2, and in darkness to evolve during the same period of time 0·33 cub. centim. of CO^2. In other words, one square metre of green surface will decompose in twelve hours of the day 63·36 cub. centims. of CO^2, and produce in twelve hours of the night 3·96 cub. centims. of CO^2.*

The quantity of carbonic acid decomposed does not represent the whole work of sunlight for the time, as water is simultaneously attacked in order to supply the hydrogen of the carbohydrates. Boussingault, in summing up the general results of his laborious researches on vegetable physiology, says, "Si l'on envisage la vie végétale dans son ensemble, on est convaincu que la feuille est la première étape des glucoses que plus ou moins modifiés, on trouve répartis dans les diverses parties de l'organisme; que c'est la feuille qui les élabore aux dépens de l'acide carbonique et de l'eau."—*Ann. de Chemie*, tom. xiii, p 415. The fundamental chemical reaction taking place in the leaf may therefore be represented as follows:

$$\begin{array}{lllllll} (1) & CO,O & + & H^2O & = & CO,H^2 & + & O,O \\ (2) & 6(CO.H^2) & & & = & C^6H^{12}O^6 & & \end{array}$$

In the first equation carbonic acid and water are simultaneously attacked, with the liberation of a volume of oxygen equal to that of the original carbonic acid, together with the formation of a substance having the composition of methylic aldehyde. The second equation represents the condensation of this aldehyde into grape sugar. The transformation induced in (1) necessitates the absorption of a large amount of energy; and if we neglect the heat envolved in the combination of nascent CO and H^2, which can be shown to be very little, the calculated result is made a maximum; whereas the condensation of (2) being attended with an evolution of heat, diminishes considerably the amount of power required. Happily Frankland's direct determination of the thermal value of grape-sugar leaves no doubt as to the true equivalent of work done in its formation. Taking the following

* The rate at which the leaf functions is dependent on the luminous intensity. The relative amounts, therefore, of carbonic acid decomposed through the action of the different colored rays are proportional to their luminous power; and the curve of assimilation is found to follow the curve of Fraunhofer. This proves that the judgment we form of equal luminous impressions is in reality due to equal mechanical effects associated with the different colored rays. Professor Draper, of New York, in his recent paper "On the Distribution of Heat in the Spectrum," by dividing the spectrum into two portions of equal luminous intensity, obtained identical thermal effects by absorption. This does not prove that each ray has the same total energy, but only that in all probability those at equal distances on either side of the mean wave-length in the normal light-spectrum of the sun are identical.

thermal values $CO, O = 68{,}000$, $H^2, O = 68{,}000$, $C^6 H^{12} O^6 = 642{,}000$, 1 cub. centim. of CO^2 decomposed as in (1) would require 6·06 gramme-units of heat, or its light-equivalent, whereas the complete change into grape sugar of the same amount of carbonic acid requires only 4·78 gramme units. But, we have seen before, 1 square decimetre of green leaf functions at the rate of 5·28 cub. centims. of carbonic acid assimilated per hour; therefore $5{\cdot}28 + 4{\cdot}78 = 25{\cdot}23$ represents the number of gramme-heat-units conserved through the absorption of light in the above period of time. Pouillet estimates the mean total solar radiation per square decimetre exposed normally to the sun's rays in or near Paris, per hour, as 6,000 gramme-units, so that $6{,}000 \div 25{\cdot}23 = \frac{1}{238}$ represents the fraction of the entire energy conserved. The estimate is by no means too great, as Boussingault has shown the leaf may function at twice the above rate for a limited time; and as both sides of the leaf are included in the measurement of the green surface in his memoir, we ought to double the fraction for a leaf exposed perpendicularly to the sun's rays, increasing the above number to the 120th part.

In connection with equation (1), above given, as representing the action of sunlight on the leaf, it is worthy of remark that, supposing the carbonic acid and water equally efficient as absorbing agents of the vibratory energy (although each has a specific absorption for certain qualities of rays), the decomposition of the two compound molecules may take place continuously side by side, owing to the equality of the thermal equivalents of carbonic oxide and hydrogen. We already know, from the laborious researches of Tyndall, how thoroughly aqueous vapor retains thermal radiations; and Janssen has further shown the same substance has a strong absorptive action on the rays of light of low refrangibility (just those rays that are in part selected by chlorophyl), producing the well known atmospheric lines of the solar spectrum. The presence, therefore, of varying quantities of aqueous vapor in the atmosphere in all probability produces a considerable difference of rate in the decomposition effected by the leaf, and may in fact end in carbonic acid and water being attacked in another ratio than that given as the fundamental equation of decomposition. Thus the same plant in different atmospheric conditions may elaborate different substances.

2. *On the Law of Extraordinary Refraction in Iceland Spar;* by G. G. Stokes, M.A., Sec. R.S. (Proc. Roy. Soc.)—It is now some years since I carried out, in the case of Iceland spar, the method of examination of the law of refraction which I described in my report on Double Refraction, published in the Report of the British Association for the year 1862, p. 272. A prism, approximately right-angled isosceles, was cut in such a direction as to admit of scrutiny, across the two acute angles, in directions of the wave-normal within the crystal comprising respectively inclinations of 90° and 45° to the axis. The directions of the cut faces were referred by reflection to the cleavage-planes, and thereby to the axis. The light observed was the bright D of a soda-flame.

The result obtained was, that Huygens' construction gives the true law of double refraction within the limits of errors of observation. The error, if any, could hardly exceed a unit in the *fourth* place of decimals of the index or reciprocal of the wave-velocity, the velocity in air being taken as unity. This result is sufficient *absolutely to disprove* the law resulting from the theory which makes double refraction depend on a difference of inertia in different directions.

I intend to present to the Royal Society a detailed account of the observations; but in the mean time the publication of this preliminary notice of the result obtained may possibly be useful to those engaged in the theory of double refraction.—*Phil. Mag.*, IV, xliv, 316.

3. *On a new Galvanic Pile, of economic construction;* by M. GAIFFE.—The high price of galvanic piles and the difficulty of procuring them being often an obstacle to the applications which might be made of them, I essayed the possibility of devising an apparatus that one could make anywhere without the aid of the professional workman, with substances of little value, widely spread in commerce, and possessing the essential quality of constancy in the effects.

The pair which, after some trials, I have adopted, resembles Callaud's in its form, used some years since on telegraphic lines; but its elements are different. It consists of a vessel into which dip two rods—one of lead, the other of zinc. The leaden one descends to the bottom; the zinc is one-half shorter. The bottom of the vessel is coated with red oxide of lead (minium); and the exciting liquid is water containing 10 per cent of chlorhydrate of ammonia.

The electromotive force of this pile is about one-third of that of a Bunsen's pair; its internal resistance is slight, and varies little; the chloride of zinc formed does not sensibly alter the conductivity of the exciting liquid; its constancy is great; finally the expense is almost nothing when the circuit is open.—*Comptes Rendus de l'Acad. des Sciences*, July 15, 1872, p 120.—*Phil. Mag.*, IV, xliv, 320.

II. GEOLOGY AND NATURAL HISTORY.

1. *Discovery of Fossil Quadrumana in the Eocene of Wyoming;* by O. C. MARSH.—An examination of more complete specimens of some of the extinct Mammals already described by the writer from the Eocene deposits of the Rocky Mountain region, clearly indicate that among them are several representatives of the lower Quadrumana. Although these remains differ widely from all known forms of that group, their more important characters show that they should be placed with them. The genera *Limnotherium*, *Thinolestes*, and *Telmatolestes*, especially, have the principal parts of the skeleton much as in some of the *Lemurs*, the correspondence in many of the larger bones being very close. The anterior part of the lower jaws is similar to that of the *Marmosets*,

but the angle is more produced downward, and much inflected. The teeth are more numerous than in any known Quadrumana. Some of the species have apparently forty teeth, arranged as follows: Incisors $\frac{2?}{2}$ canines $\frac{1}{1}$, premolars and molars $\frac{7}{7}$. A full description of these interesting remains, the first of the order detected in this country, will be given by the writer at an early day.

Yale College, Oct. 7th, 1872.

2. *Note on a new genus of Carnivores from the Tertiary of Wyoming;* by O. C. MARSH.—Additional remains of the large Carnivore described by the writer, on page 203, as *Limnofelis latidens*, show clearly that it represents a genus quite distinct from *L. ferox*. The canines and premolars of the lower jaw somewhat resemble those in the Hyæna, but there were only two incisors in each ramus. One of these is large, and close to the canine. Inside and partially behind this, is a cavity for a second and smaller incisor. The remaining teeth preserved are especially broad and massive. The first lower premolar is separated somewhat from the canine, and is inside the line of the teeth behind it. The remains now known indicate an animal about as large as a lion. The genus they represent may be called *Oreocyon*, and the type species, *Oreocyon latidens*.

Yale College, Oct. 6th, 1872.

3. *Notice of a New Reptile from the Cretaceous;* by O. C. MARSH.—An interesting addition to the Reptilian fauna of the Cretaceous shale of Kansas is a very small Saurian, which differs widely from any hitherto discovered. The only remains at present known are two lower jaws, nearly perfect, and with many of the teeth in good preservation. The jaws resemble in general form those of the Mosasauroid reptiles, but, aside from their very diminutive size, present several features which no species of that group has been observed to possess. The teeth are implanted in distinct sockets, and are directed obliquely backward. There were apparently twenty teeth in each jaw, all compressed, and with very acute summits. The rami were united in front only by cartilage. There is no distinct groove on their inner surface, as in all known Mosasauroids. The dentigerous portion of the jaw is 41·mm· in length, its depth below the last tooth is 5·mm· and below the first tooth in front 3·mm· The specimen clearly indicates a new genus, which may be called *Colonosaurus*, and the species may be named *Colonosaurus Mudgei*, for the discoverer, Professor B. F. Mudge, who found the remains in the upper Cretaceous shale of Western Kansas.

Yale College, Oct. 7th, 1872.

4. *Recent Eruption of Mauna Loa;* by Rev. TITUS COAN. (From a letter to J. D. Dana, dated Hilo, Hawaii, Aug. 27, 1872). —On the night of the 10th inst. a grand and lofty pillar of light rose from the summit of the mountain to the height of some 2,000 feet. This was directly over the great terminal crater, Mokuaweoweo. It was most distinctly seen at first from Kilauea and Kau.

On the evening of the 13th we had the first *perfect* view from Hilo. The illuminated cloud of steam and gases which hung over the crater sometimes rose in a well-defined vertical column to a great height, and then the higher portion would expand, forming an inverted cone; again it seemed lighted up above the mountain and spread out like an umbrella over the crater. The changes of form, the expansion, contraction, and convolutions of the illuminated pile, could be distinctly marked, and also the rapid variations in brilliancy dependent on the greater or less intensity of the fiery lavas in the abyss below.

It is now seventeen days since we first saw the eruption, and still the great furnace is in full blast. The action is, evidently, intense. Of all the demonstrations made in this vast cauldron on the summit of the mountain since our residence in Hilo, none have equalled this in magnitude, in vehemence, and in duration. As yet it is confined to the deep crater; and we know not whether the terrific forces now raging in this abyss will rend the walls of the mountain and let out a flow of lavas to the sea, or spend their fury within the recesses of the mountain. The scene from the border of the crater must now be fearfully grand.

I am ashamed to say, that, so far as we know, no one has yet visited the region of eruption. In spite of my age (nearly 72), were it not for the sickness in my family, I should before this have been on the summit of Mokuaweoweo. From the little ranch of Reed and Richardson in Kapapala, Kau, you can ride up on horseback in a day. I hope soon to hear that some one has been to the summit.

Ten thousand feet below the summit fires is Kilauea. This crater has also been very active of late. The south lake, which was so deep when I last wrote you, has long been filled, and it has overflowed many times, sending off broad streams of incandescent lava, filling up the great basin of 1868, elevating the southern portion of Kilauea, raising cones that puff and screech, and throw out vapor, hot gases, and sulphur. The present activity looks like some kind of sympathy with the summit furnace.

Along the shore, 4,000 feet below Kilauea, there was, on the 23d inst., a *tidal wave*. It occurred at 1 P. M. during a calm. The sea in our bay rose silently and rapidly, like an incoming tide, to the height of four feet two inches. In about six minutes it had subsided to a low point and returned again to the height of three feet. Quickly and quietly it retired again; and thus in the space of 1½ hours it made fourteen oscillations, each succeeding one growing fainter, until the sea returned to its normal condition. We had no earthquake at the time.

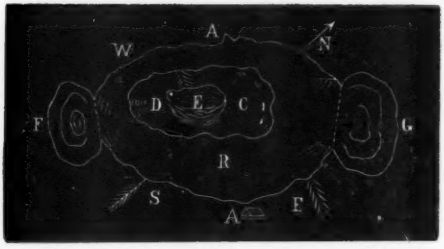
We have had occasional slight earthquakes of late, but no severe ones.

5. *Ascent of Mauna Loa to the scene of Eruption.*—The following is an extract from an account of the ascent of Mauna Loa to the place of eruption, published in the Pacific Commercial Advertiser of Sept. 21.

The summit.—Before us lay a rugged plain, about two miles in diameter, of black lava, overlaid in many places with fields of brown *a-a*, and everywhere torn into unheard of shapes by the fierce power that had upheaved the whole. To our right rose a remarkable monument or pillar showing black against the sky. On every hand yawned deep crevices, and spent waves of lava had dashed together in myriad shapes, and so congealed. Hurrying on as well as we were able, we finally reached a *cul-de sac*, formed by a branching *a-a* flow, and here we dismounted, and tethered our animals for the night. This done, we took our way five hundred yards over a narrow strip of rugged lava, and all at once stood upon the edge of the

Crater of Mokuaweoweo.—There before us, at our feet as it were, yawned a terrific chasm, with black perpendicular walls carrying the eye down some 800 feet, to where, in the inky blackness of the lower basin, sprung up in glorious sparkling light, self-born, a mighty fountain of clear molten lava.

Referring to the diagram published herewith, the reader will find that we reached the crater's edge on the eastern side at the point marked by the outline of a tent. The ancient walls that encircle the pit, marked A, on our side fell perpendicularly about five



hundred feet, while on the opposite or western side they descended nearer eight hundred, to where the plateau marked B formed a floor to the crater, broken down again to form the pit marked C. The general shape of the central crater, Mokuaweoweo, was an irregular ellipse, rather more than three-quarters of a mile through its shorter axis, by about a mile and a quarter from the dividing wall marked by a dotted line on the left, that separated it from F, the crater known as Pohaku Hanalei, to a similar though not so well defined partition wall on the right hand that joined to it the crater G. Looking straight across and below us at a distance in an air line of possibly three-quarters of a mile, there rose from a cone located near the southwest corner of the lower basin a magnificent fountain of liquid lava, about seventy-five feet in diameter, that sent its volume of brilliant sparkling molten matter to a height estimated at five hundred feet in a compact and powerful jet. The axis of this gigantic fountain was somewhat inclined toward us, so that the descending cascade fell clear and distinct from the upward shooting jet, and formed one continuous fall of liquid lava, surpassingly beautiful to gaze upon. Behind this fiery fountain, a dark incline of *débris*, partly thrown up by this

outbreak, partly formed by masses falling from the wall against which it rested, reared itself against the steep side of the crater, and stood out boldly in the intense light from the incandescent fountain. On the left, with its base nearly reached by the liquid streams flowing out from the lake into which the cascade fell, was another long pile of debris that reached to the level of the plateau B. I should say here that the sides of this plateau looked to be about one hundred feet deep to where they met the edge of the lower basin. The basin itself occupied about one-third of the space bounded by the ancient walls of the crater.

Flowing down the sides of the symmetrical cone that the falling stream of lava was rapidly forming were many bright rivers of liquid light that, spreading as they flowed away, and crossing and recrossing in a tangle of bright lines, formed a lake of rivulets that, ever widening, mingling, spreading and interlacing, presented a unique and beautiful appearance. On the extreme right hand verge of this lower basin, detached pools of fire showed that while a dark crust was forming on the surface beneath, the entire area of the basin was overflowed by the melted lava.

We watched steadily the grand fountain playing before us, and called frequently to each other to note when some tall jet, rising far above the head of the main stream, would carry with it immense masses of white-hot glowing rock which, as they fell and struck upon the black surface of the cooling lava, burst like meteors in a summer sky.

As soon as we had reached the summit level of the mountain, we heard the muffled roar of the long pent up gases as they rushed out of the opening which their force had rent in the basin's solid bed. And now that we were in full view of the grand display, our ears were filled with the mighty sound, as of a heavy surf booming in upon a level shore, while ever and anon a mingled crash and break of sound would call to mind the heavy rush of ponderous waves against the rocky cliffs that girt Hawaii.

At night the jet looked loftier, and gazing intently into the fiery column with a good glass that we had, we could see the limpid sparkling upward jet rising with tremendous force from out an incandescent lake. Following up the glowing stream, we saw it arch itself and pour over as it were in one broad beautiful cascade. While the ascending stream was almost silvery in its intense brightness, the falling sheet was slightly dulled by cooling, and thus the two were ever rising, falling, shooting up in brilliant jets, and showering down with mingled dashes of bright light and shooting spray, while in the lake out of which rose the fountain, and into which fell the fiery masses, danced and played a thousand mimic waves, and fiery foam swirled round and round. Upon its surface danced myriad jets and bubbles, and from its edge flowed out the rivulets of lava, that in a tangled maze of lines covered all the lake.

6. *Volcanic Energy: An Attempt to develop its true Origin and Cosmical Relations;* by ROBERT MALLET, F.R.S. (Proc.

Roy. Soc., No. 136, 1872). (Abstract).—The author passes in brief review the principal theories which in modern times have been proposed to account for volcanic activity.

The chemical theory, which owed its partial acceptance chiefly to the fame of Davy, may be dismissed, as all known facts tend to show that the chemical energies of the materials of our globe were almost wholly exhausted prior to the consolidation of its surface.

The mechancial theory, which finds in a nucleus still in a state of liquid fusion a store of heat and of lava, etc., is only tenable on the admission of a *very thin* solid crust; and even through a crust of about 30 miles thick it is difficult to see how surface-water is to gain access to the fused nucleus, yet without water there can be no volcano. More recent investigation on the part of mathematicians has been supposed to prove that the earth's crust is not *thin*. Attaching little value to the calculations as to this, based on precession, the author yet concludes, on other grounds, that the solid crust is probably of great thickness, and that, although there is evidence of a nucleus much hotter than the crust, there is no certainty that any part of it remains liquid; but if so, it is in any case too deep to render it conceivable that surface-water should make its way down to it. The results of geological speculation and of physico-mathematical reasoning thus oppose each other, so that some source of volcanic heat closer to the surface remains to be sought. The hypothesis to supply this, proposed by Hopkins and adopted by some, viz: of isolated subterranean lakes of liquid matter in fusion at no great depth from the surface remaining fused for ages, surrounded by colder and solid rock, and with (by hypothesis) access of surface-water, the author views as feeble and unsustainable.

A source, then, for volcanic heat remains still to be found; and if found under conditions admitting to it water, especially of the sea, all known phenomena of volcanic action on our earth's surface are explicable.

The author points out various relations and points of connection between volcanic phenomena, seismic phenomena, and the lines of mountain elevation, which sufficiently indicate that they are all due to the play of one set of cosmical forces, though different in degree of energy, which has been constantly decaying with time.

He traces the ways in which the contraction of our globe has been met, from the period of its original fluidity to the present state: first, by deformation of the spheroid, forming generally the ocean basins and the land; afterward by the foldings over and elevations of the thickened crust into mountain ranges, etc.; and, lastly, by the mechanism, which he points out as giving rise to volcanic action. The theory of mountain elevation proposed by C. Prevost was the only true one—that which ascribes this to tangential pressures propagated through a solid crust of sufficient thickness to transmit them, those pressures being produced by the

relative rate of contraction of the nucleus and of the crust; the former being at the higher temperature, and having a higher co-efficient of contraction for equal loss of heat, tends to shrink away from beneath the crust, leaving the latter partially unsupported. This, which during a much more rapid rate of cooling from higher temperature of the whole globe and from a thinner crust gave rise in former epochs to mountain elevation, in the present state of things gives rise to volcanic heat. By the application of a theorem of Lagrange, the author proves that the earth's solid crust, however great may be its thickness, and even if of materials far more cohesive and rigid than those of which we must suppose it to consist, must, if even to a very small extent left unsupported by the shrinking away of the nucleus, crush up in places by its own gravity and by the attraction of the nucleus.

This is actually going on, and in this partial crushing, at places or depths dependent on the material, and on conditions pointed out, the author discovers the true cause of volcanic heat. As the solid crust sinks together to follow down after the shrinking nucleus, the *work* expended in mutual crushing and dislocation of its parts is *transformed into heat*, by which, at the places where the crushing sufficiently takes place, the material of the rock so crushed and of that adjacent to it are heated, even to fusion. The access of water to such points determines volcanic eruption. Volcanic heat, therefore, is one result of the secular cooling of a teraqueous globe subject to gravitation, and needs no strange or gratuitous hypothesis as to its origin.

In order to test the validity of this view by contact with known facts, the author gives in detail two important series of experiments completed by him:—the one on the actual amount of heat capable of being developed by the crushing of sixteen different species of rocks, chosen so as to be representative of the whole series of known rock formations from Oolites down to the hardest crystalline rocks; the other, on the co-efficients of total contraction between fusion and solidification at existing mean temperature of the atmosphere of basic and acid slags, analogous to melted rocks.

The latter experiments were conducted on a very large scale, and the author points out the great errors of preceding experimenters, Bischoff and others, as to these co-efficients.

By the aid of these experimental data, he is enabled to test the theory produced when compared with such facts as we possess as to the rate of present cooling of our globe, and the total annual amount of volcanic action taking place upon its surface and within its crust.

He shows, by estimates which allow an ample margin to the best data we possess as to the total annual vulcanicity of all sorts of our globe at present, that less than one fourth of the total heat at present annually lost by our globe is upon his theory sufficient to account for it; so that the secular cooling, small as it is, now going on is a sufficient *primum mobile*, leaving the greater portion

still to be dissipated by radiation. The author then brings his views into contact with various known facts of vulcanology and seismology, showing their accordance.

He also shows that to the heat developed by partial tangential thrusts within the solid crust are due those perturbations of hypogeal increment of temperature which Hopkins has shown cannot be referred to a cooling nucleus and to differences of conductivity alone. He further shows that this view of the origin of volcanic heat is independent of any particular thickness being assigned to the earth's solid crust, or to whether there be at present a liquid fused nucleus, all that is necessary being a *hotter* nucleus than crust, so that the rate of contraction is greater for the former than the latter. The author then points out that, as the same play of tangential pressures has elevated the mountain chains in past epochs, the nature of the forces employed sets a limit to the height of mountain possible of the materials of our globe.

That volcanic action due to the same class of forces was more energetic in past time, and is not a uniform but a decaying energy now. Lastly, he brings his views into relation with vulcanicity produced in like manner in other planets, or in our satellite, and shows that it supplies an adequate solution of the singular and so far unexplained fact that the elevations upon our moon's surface, and the evidences of former volcanic activity, are upon a scale so vast when compared with those upon our globe.

Finally, he submits that if his view will account for all the known facts, leaving none inexplicable, and presenting no irreconcilable conditions or necessary deductions, then it should be accepted as a true picture of nature.

7. *Solvent action of water.* From the Anniversary Address of J. Prestwich, President of the Geological Society, February, 1872.—Let us now look at the geological bearing of the question connected with the solvent action of the water on the strata it traverses. The analyses, made for the Commission by Drs. Frankland and Odling, of the waters of the Thames and its tributaries in the Oolitic and Chalk area, show that the rain-water has taken up of solid matter in every 100,000 parts or grains a quantity varying from 25·58 to 32·95 parts or grains, or an average of 29·26, which is equal to 20·48 grains per gallon; another analysis of the Thames water at Ditton gives 20·78 grains per gallon of solid residue. It was also shown by Drs. Letheby and Odling and Professor Abel that the unfiltered waters of the Thames Companies, which take their supplies above Kingston, contained 20·82 of solid residue. If from the average of 20·68 we deduct 1·68 grain for organic and suspended matter, we have 19 grains of dissolved inorganic matter for every gallon of water flowing past Kingston. This is of course apart from the sediment carried down in floods. The ordinary monthly analysis, conducted by the same eminent chemists during the course of several past years, shows that this quantity is liable to very little variation, the only difference being that it is somewhat larger in winter and less in summer.

Some general estimates have already been made by Professors Ramsay and Geikie of the quantity of mineral matter carried down in solution by the Thames; but the more exact data supplied to the Commission enables us to make some additions to previous results. Taking the mean daily discharge of the Thames at Kingston 1250 million gallons, and the salts in solution at 19 grains per gallon, the mean quantity of dissolved mineral matter there carried down by the Thames every twenty-four hours is equal to 3,364,286 lbs. or 1502 tons, which is equal to 548,230 tons in the year. Of this daily quantity about two-thirds, or say 1000 tons, consist of carbonate of lime, and 238 tons of sulphate of lime; while limited proportions of carbonate of magnesia, chlorides of sodium and potassium, sulphates of soda and potash, silica and traces of iron, alumina and phosphates constitute the rest. If we refer a small portion of the carbonates, and the sulphates and chlorides chiefly, to the impermeable argillaceous formations washed by the rain-water, we shall still have at least 10 grains per gallon of carbonate of lime, due to the Chalk, Upper Greensand, Oolitic strata, and Marlstone, the superficial area of which, in the Thames basin above Kingston, is estimated by Mr. Harrison at 2072 square miles. Therefore the quantity of carbonate of lime carried away from this area by the Thames is equal to 797 tons daily, or 290,905 tons annually, which gives 140 tons removed yearly from each square mile; or, extending the calculation to a century, we have a total removal of 29,090,500 tons, or of 14,000 tons from each square mile of surface. Taking a ton of chalk, as a mean, as equal to 15 cubic feet, this is equal to the removal of 210,000 cubic feet per century for each square mile, or of $\frac{9}{100}$ of an inch from the whole surface in the course of a century, so that in the course of 13,200 years a quantity equal to a thickness of about 1 foot would be removed from our Chalk and Oolitic districts.

8. *Correlations of the Coal Measures of Britain, France and Belgium.* From the Anniversary Address of the President of the Geological Society, Joseph Prestwich, F.R.S., February, 1872.—It may be asked if any correlation can be established between the coal measures of Bristol and South Wales, and those of France and Belgium. So far as the identity of any particular bed of coal or of rock, it is impossible, and we should not expect it; for the variation in all the beds of any coal basin is well known to be so great and rapid that in the different parts of the same basin it is often difficult, and sometimes impossible, to establish any correlation, while in adjacent basins, such as those of Wales and Bristol, or of Hainaut and Liége, such attempts have, with few exceptions, hitherto utterly failed. There are, however, general features which serve to show some relationship. The great dividing mass of from 2000 to 3000 feet of rock called Pennant exists in both the Welsh and Bristol coal field; and the total mass of Coal-measures is not very different, it being, say, 10,500 feet in the one, and 8500 in the other, and there being in Wales 76, and in Somerset 55 workable seams of coal. In the Hainaut (or Mons

and Charleroi) basin, the measures are 9400 feet thick, with 110 seams of coal; in the Liége basin 7600 feet, with 85 seams; and in Westphalia 7200 feet, with 117 seams. On the other hand, none of our central or northern coal basins, with the exception of the Lancashire field, exceed half this thickness, and more generally are nearer one fourth. Further, the difference which exists between the northern coals and those of Wales and Somerset, the preponderance of caking coals in the north, and of anthracite, steam, and smiths' coal in the south, equally exists between our northern coals and those of Belgium, which latter show, on the one hand, close affinities with those of Wales and Bristol. I am informed by two experienced Belgian coal-mining engineers and good geologists, who have twice visited our coal districts, that the only coals they found like those of Belgium were the coals of South Wales and Radstock; there was the same form of cleavage, the same character of measures, and the same fitness for like economical purposes. Organic remains afford us a little help; but not sufficient is yet known of their relative distribution. The plants are, as usual, the same; so also are shells of the genus *Anthracosia*, and a number of small Entomostraca; while there is a scarcity of the marine forms which are more common in some of our central and northern fields. That, therefore, which best indicates the relation between the coal fields of the southwest of England and those of the north of France and Belgium, is the similarity of mass and structure, uniformity of subjection to like physical causes, and identity of relation to the underlying older and to the overlying newer formations.

It was in the north that the conditions fitted for the formation of coal first set in. The common *Stigmaria ficoides* and various Coal-measure plants appear at the base of the Carboniferous or in the Tuedian series of Northumberland, which there overlies conformably the Upper Old Red Sandstone; and productive beds of coal exists low down in the Mountain-Limestone series. These disappear in proceeding southward, and the great productive coal series becomes confined to beds overlying the Millstone Grit. If the coal growth set in earlier in the north, it seeme to have been prolonged further south, under more favorable conditions, to a later period. What those conditions were—whether the proximity of a greater land-surface, of a longer and greater subsidence, with more numerous rests—we cannot yet pretend to say.

9. *Recent Observations in the Bermudas;* by MATTHEW JONES. —As my late visit to these islands has placed me in possession of facts relating to their original aspect of a somewhat conclusive nature, I deem it advisable to communicate such in a brief form, instead of awaiting the time requisite for the preparation of a more elaborate paper on the subject.

On previous occasions I have always regretted my inability, from lack of time, to look more closely into their geological character in the hope of discovering some satisfactory clue to their primitive condition. I was aware that in different parts of the

islands road cuttings and well borings had revealed layers of red earth at certain depths below the surface, the consistence of which was similar to that now forming the present surface soil, and it did not require much force of imagination, after personal inspection, to conceive that such layers of red earth were first formed by the decomposition of vegetable matter which grew upon former surfaces, and became covered to their respective depths by accumulated masses of drift sand, which from natural causes hardened into more or less compact sandstone. But these different layers were but a few feet beneath the surface, and so, although interesting as throwing light upon the gradual elevation of the land by drift material forming over them, yet they afforded no evidence of a contrary nature—viz: the *submergence* of the Bermuda group. Indeed, I have always been led to suppose from appearances that the whole group was the result of an upheaval of the ocean bed slightly above the water level, and a gradual elevation afterward by means of drift matter aided by the consolidating agency of reef-building zoophytes encircling the whole with a barrier reef, and by isolated patches gradually filling up the space within. The investigations, however, which I have recently been able to make, tend I think to prove that the barrier reef encircling the islands, which has hitherto been considered an atoll, is merely the remnant of the more compact calcareous rock which formed the shore of a much more extensive island group than that now existing.

My views in this respect are borne out by the following facts:—The barrier reef, as far as I have inspected it, is merely ordinary calcareous rock coated with Serpulæ, Nullipores, &c., the reef builders working only in the sheltered waters between the reef and the shore in three to eight fathoms. About two years ago submarine blastings were carried on at the entrance of Hamilton Harbor, and at a depth of over six fathoms a cavern was broken into which contained stalactites and red earth. Again within the last few months, I have, through the kindness of his Excellency Major-General Lefroy, C.B., F.R.S., the present Governor, been placed in possession of still more satisfactory information. During the past two years extensive submarine blastings have taken place inside an artificial harbor, situated at the western extremity of the islands, for the purpose of forming a bed of sufficient depth for the reception of the "Great Bermuda Dock," which attracted so much attention off Woolwich when launched some three or four years ago. The excavations extended to a depth of fifty-two feet below low water mark. At forty-six feet occurred a layer of red earth two feet in thickness, containing remains of cedar trees, which layer rested upon a bed of compact calcareous sandstone. Here we have the first satisfactory evidence of the submergence of an extensive deposit of soil once upon the surface, and that to the depth of forty-eight feet below the present low water level, which consequently grants an equal elevation above it in former times. Now, on carefully surveying the Bermuda

chart, we find that an elevation of forty-eight feet will bring the whole space which intervenes between the present land and the barrier reef, now covered with water, above the water level. This attained, what more is required to prove the former extent of the island group before the present submergence to the present barrier reef? But having clearly ascertained beyond doubt that the Bermudas were once forty-eight feet higher than at present, will any one be bold enough to deny them a greater elevation? I have reason to believe that they once extended in a south-westerly direction—not only out to the reef, but to a greater distance. There are some rocky ledges about twenty to twenty-five miles from land in that direction, known as "The Flatts," lying in about thirty five to forty fathoms water; and, singularly enough, in the very oldest maps of the Atlantic, copies of which I have consulted in the British Museum, "The False Bermudas" are put down about this position. Is it unreasonable to suppose that a low lying group of islets did actually exist here in former times? Again, in Smith's "History of Virginia," which gives an excellent account of the islands in the early part of the seventeenth century, it is stated, among other notes upon their natural history, that flocks of crows, no doubt the same species (*Corvus Americanus*) which now inhabits them, were in the habit every evening of winging their flight from the main island toward the north. This observation, which from its simplicity I should the more readily believe to be a true statement, would clearly prove the existence of land in that direction at no great distance; for the habit of this bird to leave its roosting place for distant feeding grounds during the day, to return at random, is one of its well-known characteristics.

Taking these matters into consideration, I see everything to support the supposition that the Bermudas once presented a much more extensive aspect than they do at present, and certain additional evidences which I hope to bring forward shortly in a collected form, will, I conceive, tend to confirm my impression that the restricted terraqueous area lying within the limits of the outer barrier reef is merely the summit of one of a range of islands which extended in somewhat semicircular form for a distance of seventy or eighty miles, and which have suffered submergence to a depth only to be correctly ascertained by borings, which might be successfully accomplished under the auspices of the Government at a trifling expence.—*Nature, Aug.* 1.

10. *History of the names Cambrian and Silurian in Geology*, by T. Sterry Hunt. 64 pp. 8vo. From the Canadian Naturalist for April and July, 1872.—Prof. Hunt has here made a valuable contribution to historical geology. But the conclusion of the whole matter that the name Cambrian should be now used in this and other lands for the Primordial or part of it, because this would be in accordance with "historic truth," does not seem necessarily to follow.

In England, the so-called Cambrian has turned out, as Hunt recognizes, Primordial in its upper half at least—a part now called

the Menevian group,* with the underlying Harlech grits; that is, it contains Paradoxides and the same range of generic forms that Barrande and others had previously found in the European Primordial; and a transfer, therefore, of all to the Primordial was natural. But Prof. Hunt says, after alluding to the views of some others, that Barrande's course "is a still greater violation of historic truth," as if error which history had made venerable might never be eradicated from science.

The term Silurian, as used in Great Britain, has included a wide range of formations, from the Lingula Flags to the top of the Ludlow group, and all this in spite of a wide range in the tribes of fossil species, and notwithstanding the unconformability between the Upper and Lower Silurian. Now the Lingula Flags pass into the Cambrian without break or unconformability, and with but a small change in the life. What good scientific reason is there for cutting off this comprehensive division of geological time, the Silurian, at a point both stratigraphically and paleontologically unimportant? Why should Murchison's or Sedgwick's determination of the limits of the Silurian, made in an early stage of the science, have anything more than a historical interest? To throw the Lingula flags down into the Cambrian, as Lyell has done, is violating "historic truth" as much as to throw the Primordial Cambrian beds up. "Historic truth," in fact, has little weight in the question, though important as regards the labors of two eminent English geologists. Whichever course best exhibits the system of geological truth should be the one adopted by the science. If the term Cambrian has advantages over Primordial sufficient to make its substitution for the latter desirable, that will take place, whatever the past may say; but otherwise, not. In a similar manner, if the distinction between a *Paradoxides* and an *Olenus*, and other differences less important between the living species of these groups, is not enough to demand that the Primordial (or Cambrian) should be separated from the Silurian and be made a separate and equivalent grand division in the system, it should not be done whatever the authority for it.

11. *Report of the Geological Survey of the State of New Hampshire*, showing its progress during the year 1871; by C. H. Hitchcock, Ph.D. 56 pp. 8vo. Nashua, N. H., 1872.—Prof. Hitchcock has made great progress during the year in determining the distribution of the Labradorite and other rocks of the White Mountain region, and is throwing much light on this dark and most difficult part of American geology. His report is accompanied by a colored map, showing the areas occupied by the different kinds of rocks. There are also descriptions of the

* Some of the species described from it by Salter are *Paradoxides Davidis, P. Aurora, P. Hicksii, Anopolenus* (near Paradoxides) *Henrici, A. Salteri* Hicks, *Conocoryphe* (Conocephalites) *variolaris, C. Bufo* Hicks, *C. applanata, C.* (?) *humerosa, Holocephalina* (near Conocoryphe) *primordialis, Agnostus princeps, Microdiscus punctatus, Leperditia Solvensis* Jones, *Theca corrugata, Protospongia fenestrata.* See further, Quart. Jour. Geol. Soc., xx, 233, xxi, 476, xxv, 51, xxviii, 173 and Rep. Brit. Assoc. for 1865, 1866 and 1868.

rocks, and a number of analyses of the contained feldspar. The Report also contains very valuable tables of heights along different railroads, and in the mountains. There are brief accounts also of iron-ore in Bartlett, and the alluvial gold of Indian Stream. The preparation of the final quarto Report on the geology of the State is stated to have been begun.

12. *Memorie per servire alla Descrizione della Carta Geologica d' Italia*; publicate a cura del R. Comitato Geologico del Regno. Vol. I. 364 pp. 8vo.—A beautiful volume of Italian geological memoirs, illustrated by a geological map of the Island of Elba, and many plates of sections and fossils. The figures of Tertiary fossils fill seven quarto plates, and are admirably engraved.

13. *On the Occurrence of Native Sulphuric Acid in Eastern Texas;* by J. W. MALLET, Ph.D. (Proc. Brit. Assoc., 1872.)—Not far from the Gulf of Mexico, and within twenty-five or thirty miles to the westward of the Neches river, there occur at several localities—in some instances in the woods, in others in the midst of open prairie—small drainage-wells and shallow pools of water strongly sour to the taste. This sourness is due to the presence of free sulphuric acid, which is accompanied by various salts, especially aluminium and iron sulphates. At most of these points gases are continually escaping (hydrogen sulphide, marsh gas, and carbonic anhydride), the bubbles burning readily on the application of a light.

At the bottom of the water in some instances, as at one point where, by means of an artificial bank, a pond has been formed of some 250 feet in diameter, known locally as the "sour lake," an earthy crust with intermingled free sulphur is observable. A thick, tarry variety of petroleum is found oozing from the surrounding soil, occasionally to such an extent that sods taken up with a spade are set on fire and used to give light in the open air at night.

At a point in Louisiana some fifty or sixty miles further east, where, however, the acid water does not occur, though combustible gas and petroleum are met with on the surface, a most remarkable bed of native sulphur, 100 *feet in thickness*, has been reached at the depth of 450 feet by boring, and a shaft is being at present sunk for its exploitation. This large mass of native sulphur is more or less mingled with calcium carbonate, and underlaid by gypsum. The circumstances connected with the occurrence together in this region of combustible gases, petroleum, sulphur, sulphuric acid, and gypsum are of great interest in relation to the mineral history of native sulphur.

The sulphuric acid water, which seems to be probably altogether of superficial origin, is worthy of notice from the unusual strength occasionally attained. The water varies very much at the different localities and at different times. In one instance, a specimen examined by Dr. Mallet contained no less than 5·290 grms. of free sulphuric acid (H_2SO_4) to the litre, or 370 grs. to the imperial gallon, this exceeding any amount hitherto reported from

other localities, unless the acid spring of the Paramor de Ruiz, in New Granada, be an exception, examined by Lewy, who does not state precisely how much of the very large quantity of sulphuric acid found is uncombined with bases. The water of the Rio Vinagre, flowing from the volcano of Purace in the Andes of Popayan, as described by Humboldt and Boussingault, contains only 1·11 of free sulphuric acid (SO_3?) in 1000 parts of the water, with 0·91 of hydrochloric acid.

14. *Analysis of a Compact Talc from North Carolina;* by Mr. J. B. ADGER, of the Laboratory of the University of Virginia. (Chemical News, No. 654.)—Among the minerals referred to there was a very beautiful "soapstone, from the Nantahela Mountains, 8 miles from the mouth of Nantahela River, Swayne Co. (formerly Cherokee Co.), N. C." It had been sawn into slabs of about 1½ inches thick, was very uniform in character, compact, with indistinct traces of foliated structure, white with a faint greenish shade, lustre pearly, streak white, moderately translucent, greasy to the touch, hardness = about 1·25, sp. gr. = 2·82. It resembled somewhat the finer and light-colored specimens of Chinese jade or nephrite. Analysis afforded: Silica 57·72, magnesia 33·76, alumina 2·52, iron monoxide 0.64, water 6·01=100·65.

If the silica, magnesia, and water, alone be considered, the above numbers correspond pretty closely to the formula $(\frac{5}{6}MgO + \frac{1}{6}H_2O), SiO_2 + \frac{1}{6}H_2O$.

The mineral is obviously distinct from the foliated talc from Webster, Jackson Co., N. C., analyzed by Genth (this Jour., II, xxxiii, 200, as quoted in Dana's "Mineralogy," 5th ed., p. 453). In the latter 0·23 per cent of nickel oxide was found, and but 0·34 per cent of water.

In the mineral now described there is no nickel.

University of Virginia, March 12, 1872.

15. *Leucite.*—Prof. vom Rath, the excellent crystallographer of Bonn, has found, through the examination of a twin crystal, as well as by measurements, that the crystals of leucite, instead of being isometric trapezohedrons, are really tetragonal.

16. *On the occurrence in recent Pine timber of Fichtelite, a hydrocarbon hitherto known only in a fossil state;* by J. W. MALLET, Ph.D. (Proc. Brit. Assoc., 1872.)—Some nearly colorless crystalline crusts, found in clefts between the annual rings of growth of a log of long-leafed pine (*Pinus Australis*) in Alabama, were found to dissolve in boiling alcohol (more easily in ether), and, on cooling, to crystallize in monoclinic forms with greater distinctness. A specimen was exhibited of this material purified by two or three re-crystallizations; it had been found to agree perfectly in physical and chemical properties with the fichtelite of Bromeis and Clark, and on analysis yielded—

Carbon	87·82
Hydrogen	11·91=99·73

agreeing with the formular $x(C_5H_8)$. The fusing-point was found =45°C.

17. *Botanical Publications and Intelligence.*—Among the publications that have come to hand the most important to American botanists is the

Genera Lichenum, an Arrangement of the North American Lichens, by Edward Tuckerman, M.A. Amherst: Edwin Nelson. 1872. pp. xv, 281, 8vo.—This small volume contains the mature and long-considered results of many years of earnest study, and has been retarded and (as respects its introductory chapter) curtailed by serious illness brought on by overwork upon it. Having carried it through the press, the author is seeking repose and complete restoration in the Old World, whither our best thanks and wishes, and those of all his botanical associates, follow him. We hope for his return in due time, in full vigor, and that the "Synopsis of the North American Lichens, which is in preparation, but for the present necessarily laid aside," may be resumed and completed. Then our students of lichens, and those who would fain be such, will for the first time be supplied with text books for the study, and those of the very highest order of merit. The present work, addressed to lichenologists and necessarily critical, not to say recondite, needs to be supplemented by the Synopsis, which will be a practical guide to the beginner. We are incompetent to criticize this volume; but we may say that the print and paper are truly excellent, and if, as the imprint indicates, the composition and press-work were done in a country office, it is wonderful.

The Flora of British India, by J. D. Hooker, C.B., &c., assisted by various Botanists. Part I. pp. 208, 8vo. London: L. Reeve & Co. 1872.—The Indian Flora is here begun again, in a form and scope and with a vigor which render its completion hopeful. "The originally contemplated and more extended plan," exemplified in the first and only volume of Dr. Hooker and Dr. Thomson's *Indian Flora*, being abandoned, on account of the long-continued ill-health of the latter and the manifold other duties of the former, the task is now taken up more advantageously in a form like that of the other British Colonial floras, but still more condensed, indeed, nearly on the model of Dr. Hooker's Flora of the British Islands. This gives on the average between three and four species to the page. The references are sufficient; and the terse characters are more serviceable than long descriptive phrases. The work will be voluminous enough when the 14,000 Indian species are described in it. The present half volume begins with *Ranunculaceæ* and ends with *Polygalaceæ*. Dr Thomson's name is associated with the editors in the earlier orders; Dr. Anderson's in *Cruciferæ*;. and the *Polygalaceæ* are elaborated by Mr. A. W. Bennett.

Grevillea, a Monthly Record of Cryptogamic Botany and its Literature; edited by M. A. Cooke, M.A. Williams & Norgate, 8vo. In parts of 16 pages; sixpence a number; five shillings a year.—Three numbers, July—September, are before us; each has a plate, colored or plain; and, although Fungi naturally predomi-

nate in the letter-press, the other lower Cryptogams have their share of attention. The characters of the *Fungi* described by Mr. Peck in the 23d Report of the Regents of the University of the State of New York, are here reproduced. A series of articles by the veteran mycologist Berkeley, describing North American Fungi, begins in the third number. In fact, American Cryptogamic Botany is likely to receive no small share of attention, and we would warmly commend this little journal to all who in this country are interested in these subjects. We think the work will prove worthy to bear the honored name of one of the most worthy of British Cryptogamists of the last generation.

Linnæa.—The third volume of the new series, edited by Garcke (of which three fascicles are at hand), continues Böckler's long account of the *Cyperaceæ* of the Berlin herbarium, and also the late Dr. Rohrbach's papers on *Caryophylleæ*. The portion of the latter now published relates mainly to the *Alsineæ* of the New World, from Mexico southward, and is edited by Dr. Garcke. It is seldom wise to print posthumous manuscript which is not fairly completed by the author. For instance, *Lastarriæa* of Remy is here included in the *Illecebreæ* or *Paronychieæ*, with the remark that, although Remy, with all the characters before him, had referred the genus to the *Polygonaceæ*, Mr. Bentham had correctly transferred it to the former order, and had even been anticipated by Kunze and (in MSS.) by Kunth. If Rohrbach, and still more, Bentham, had really noticed the orthotropous ovule and seed, they could hardly have failed to see the Polygonaceous character. The genus, as we have elsewhere indicated, is as it were a *Chorizanthe* without a distinct gamophyllous involucre, but with perianth imitating it.

Casimir DeCandolle, in the third fascicle, publishes descriptions of new *Piperaceæ* which have come to his notice since the publication of that order in the Prodromus. It begins with "*Anemiopsis Bolanderi*," from California, founded on specimens from Dr. Bolander which are yet unknown to us.

The *Flora Brasiliensis*, continued by Professor Eichler, has Dr. Masters' revision of the *Passifloraceæ* for the 57th fascicle; and this completes vol. 13, part 1, with index, &c. The synopsis of all the known American species is a great help. Fascicle 58, which completes the second part of vol. 14, contains the *Phytolaccaceæ*, by J. A. Schmidt of Hamburg, and the *Nyctagineæ* by the same; also the *Crassulaceæ* (only two indigenous species) and *Droseraceæ* (a dozen of *Drosera*) by the editor himself. He remarks (from Caspary) that in all the pentamerous species of *Drosera* the second sepal faces the axis, while in *Aldrovranda* the sinus between the second and fourth is in this position, the third being superposed to the bract. *D. intermedia* (i. e., *D. longifolia* L., var. *Americana*) extends to Brazil, where *D. graminifolia* represents our northern *D. filiformis*. Fascicle 59 is a very thin one; it contains only the *Equisetaceæ*, which were elaborated by the late Prof. Milde, and the two sheets were printed more than

two years ago. They have been awaiting Prof. Braun's account of the *Rhizocarpeæ* and *Isoetes*, which, when issued, will complete the volume of the higher Cryptogamia.

Prof. Hofmeister has accepted the botanical chair at Tübingen, vacated by the death of Von Mohl; *Dr. Sachs* succeeds Hofmeister at Heidelberg; and *Dr. Hegelmaier* is the new professor at Kiel. Prof. Kerner has been transferred from Innspruck to the University of Prague. By a misprint in the July number of this Journal, the name of the late *Dr. Wight* was given as Dr. Wright.

Herbarium of the late Dr. Curtis.—A biographical notice of the late Dr. M. A. Curtis, which has been already too long defered, must now be postponed to the January number. His herbarium is to be sold by his family, under directions from Dr. Curtis that it shall be kept together if practicable. Clearly this collection ought to be retained in North Carolina, being authentic for the flora of that State and the exponent of his full catalogues and descriptions of the plants of North Carolina, published by the State Government. A moderate sum would probably secure it, and would be most worthily invested to that end. If the State should not acquire it, or if it should be found necessary to divide it, the collection of *Fungi* ought to find a ready sale, as it contains the types of the hundreds or even thousands of species which Dr. Curtis has described, or at least determined and catalogued. Its loss to science, or even to this country, would be irreparable.

Bentham and Hooker's *Genera Plantarum*, vol. 2.—We have seen more of the earlier sheets of this volume, the first part of which may be expected at the close of the year. A. G.

17. *A Handbook of Chemical Technology;* by RUDOLPH WAGNER, Ph.D., Professor of Technology at the University of Wurtzburg. Translated and edited from the eighth German edition, with extensive additions, by William Crookes, F.R.S. Large 8vo, pp. xvi, 745, xvi. New York, 1872. (D. Appleton & Co.)—Professor Wagner's name has become universally known by his excellent Jahresbericht der chemischen Technologie, which first appeared in 1856. The first edition of the work before us was issued in 1850, and a new edition has since appeared regularly every three years, constantly enriched and improved by the progress of discovery. The eminence of the author in his chosen field ensures a valuable volume. And, although, from the necessities of the case, it must be somewhat cursory in its plan, since it is obviously impossible to get the whole of applied chemistry into such a volume, yet we are agreeably disappointed at the number of the subjects considered and the thoroughness with which they are treated. Mr. Crookes deserves praise, also, not only for the excellence of his translation, but for the original matter he has added. Now that we have in English so good a text-book upon Chemical Technology, we trust that more attention will be paid to teaching it systematically in our schools of science.

18. *On Beavers and Beaver Dams in Mississippi;* by Mr. JOHN SHELTON. (From a letter to one of the editors, dated Raymond, Hinds Co., Mississippi, Sept. 12.)—I have resided in this

county since 1837, now for nearly thirty-five years. When I came here I was young and somewhat given to hunting. At the outset, to my inquiries of other hunters, whether there were beavers here, it was replied, that there were a few, but no one could then tell me where there was one of their dams in this neighborhood.

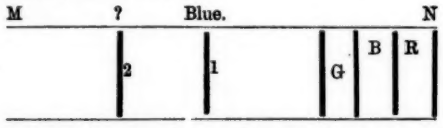
And yet by the year 1850, their dams were to be found in nearly all the streams in the county, that were not so small as to become dry during our long summers, or two large for the operations of the beaver. They continued to increase, greatly to the injury of most of our low land, and to the annoyance of its cultivators. In 1858 or 1859, a professional trapper from Wisconsin, if I am not mistaken, caught seventy-five or eighty beavers in this county in less than a month's time.

They are yet increasing in this county, as I have no doubt they are in all the counties of central Mississippi and Alabama, and perhaps entirely throughout both States. I have no doubt that, in Hinds county, they are more than half as numerous as the population. I now write in the Court House of the county, and they can be found in sight of it, and at a less distance than one mile.

III. Astronomy.

1. *Spectrum of the Aurora;* by Edward I. Holden, 2d Lieut. of Engineers. (From a letter to the editors, dated West Point, N. Y., Oct. 14, 1872.)—I have this evening succeeded in observing the spectrum of a very fine aurora, which appeared about 7 P. M., and lasted perhaps 20 minutes. It first appeared as a rosy cloud about 15° wide and perhaps 30° high, bearing N. 30° W. by compass. Afterward it spread to the zenith, and was principally in the shape of a band, of (say) 15° wide extending from the N.W. to the E. No pulsations of any magnitude were evident, but a radiated structure was manifest.

The spectroscope (pocket, by Hawkins & Wales) was first turned on the full moon, and an idea of the length of the spectrum obtained; then with a wide slit it was turned on the aurora, and the following sketch made, which was *carefully verified*, so that it represents exactly what I saw.



The length MN is what I conceived to be the length of the spectrum given by my instrument under usual conditions. The violet (extreme) rays seemed cut off, and I saw 1° a broad and bright *red* band (R), 2d a black space equal in width to it (B), 3d a green and bright band (G) nearly as wide, then a faint spectrum of diffused light, and a bright line in the blue (1), then a bright line more refrangible but whose color could not be definitely *seen* (2). The relative distances for my instrument are kept in the drawing.

I then opened Angström's "Spectre Normal," and saw that he gave the auroral line as in the yellow. I observed this green line again, and cannot persuade myself that it was yellow. The black space I am *sure* of; and it was also seen plainly by an inexperienced person, into whose hands I put the instrument. The slit was then narrowed and turned on the moon, and adjusted to give the Fraunhofer lines most clearly. The aurora by this time was fainter, and I can only be sure of a bright line (green) with a suspicion of my former blue line. Opening the slit again, the red band of the diffused light spectrum was *close* against the green bright line. The aurora then faded. I mention this black space as it is not what I expected to see from my reading of Angström and Winlock.

IV. Miscellaneous Scientific Intelligence.

1. *Institute of Technology, Boston.*—Prof. T. Sterry Hunt, for twenty-five years connected with the Geological Survey of Canada, has entered upon his duties as Professor of Geology in the Institute of Technology.

2. *Annual Rep. of the Director of the Meteorological Observatory, Central Park, New York.* 42 pp. 8vo.—At the close of this excellent Report there is a series of synoptic charts, one for each month of the year, giving the mean height of the barometer, that of the thermometer, and the strength of the wind for the month.

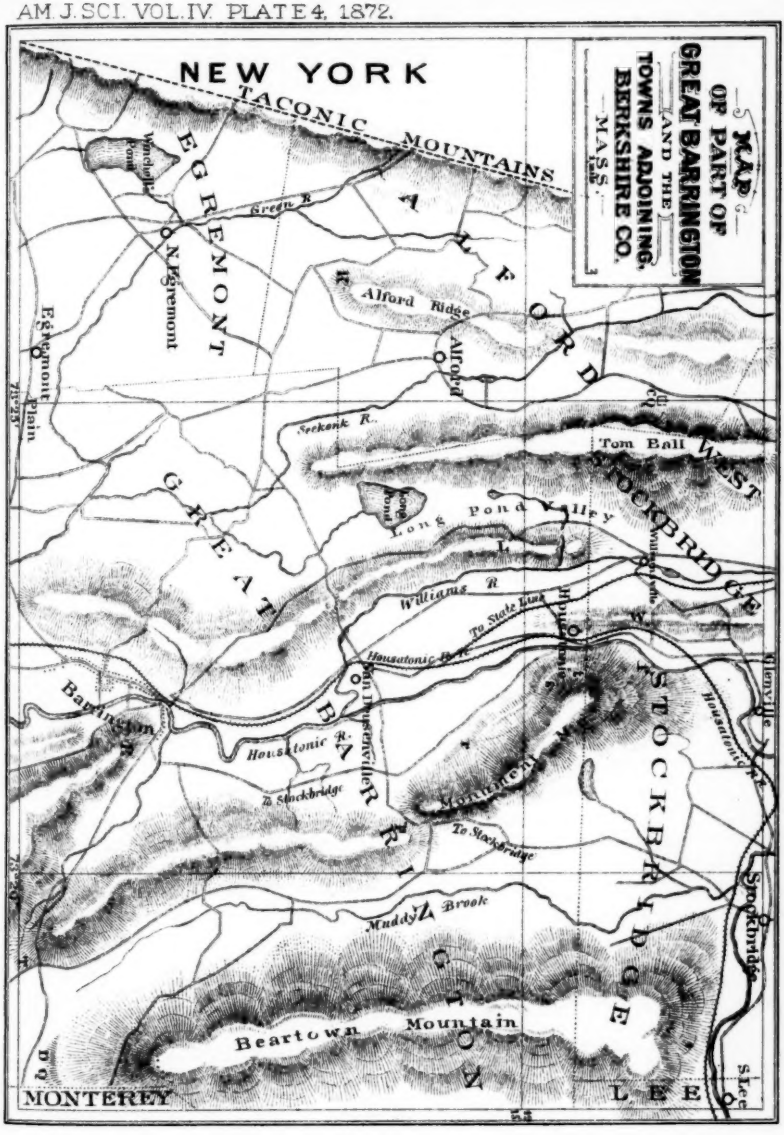
3. *Hayden's Geological Exploration in the Rocky Mountains.*—A letter from Dr. Hayden, dated Gallatin City, Montana, Oct. 10, states that his two parties have been successful in their work at every point; and that no accident or sickness has occurred. Dr. Hayden's branch of the exploration will have for the Report a map of a territory 10,000 square miles in area, with contour lines of 100 feet.

Obituary.

Rev. John B. Perry.—Professor Perry, in charge of the Department of Primordial Geology in Harvard College, died on the 30th October, in his 47th year. He had recently returned from the meeting of the American Association at Dubuque. Prof. Perry was a graduate of the University of Vermont. His residence at Swanton, in Northern Vermont, as pastor of a church, led to his examining into its geology, and especially the so-called Taconic rocks of that region, and several papers were published by him on the subject, some of which appeared in this Journal. He was greatly interested in his favorite science, and active as an observer.

John F. Frazer, LL.D., Professor of Natural Philosophy and Chemistry in the University of Pennsylvania, died on Saturday afternoon, at a quarter before three o'clock, of heart disease. A further notice will be given in our next issue.

On the Geology of Lower Louisiana and the Salt Deposit on Petite Anse Island; by Eugene W. Hilgard, Ph.D., Prof. Chem., Univ. Mississippi. 34 pp. 4to. No. 248 of the Smithsonian Contributions to Knowledge.


MAP
OF PART OF
GREAT BARRINGTON
AND THE
TOWNS ADJOINING,
BERKSHIRE CO.
MASS.
NEW YORK
TACONIC MOUNTAINS
EGREMONT
GREAT
HOOSAC
RANGE
STOCKBRIDGE
WEST
Windsor Pond
Green R.
N. Egremont
Egremont Plain
Alford Ridge
Alford
Seekonk R.
Tom Ball
Long Pond
Long Pond Valley
Williams R.
To State Line
Housatonic R.
Barrington
Housatonic R.
To Stockbridge
Monument Mt.
To Stockbridge
Muddy Brook
Bear Mountain
Glendale
Housatonic R.
Stockbridge
MONTEREY
LEE